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CAMBRIDGE MATHEMATICAL STUDIES

AND THEIR RELATION TO MODERN PHYSICAL SCIENCE

CHALLIS

REMARKS

ON THE

CAMBRIDGE MATHEMATICAL STUDIES,

AND

THEIR RELATION TO MODERN PHYSICAL SCIENCE.

BY THE

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PREFACE.

WITHIN the last forty years several works on the Cambridge Mathematical Studies have been produced by members of the University who had taken an active part in improving and extending them either by teaching or writing, and were in other ways well qualified to form a correct opinion respecting their character and tendency. These works were specially called for in consequence of various changes which it had been thought desirable to make in the system of University instruction, in respect both to the modes of teaching and examining, and the subjects taught. In the year 1837 a work was published by Whewell entitled "On the principles of English University Education," and in 1845 another of like character was produced by the same author, the full title of which is, "Of a Liberal Education in general, with particular reference to the leading studies of the University of Cambridge." As might be expected from the titles, so much of these works as is devoted to mathematical studies treats

of them mainly as *educational* means ; and accordingly very much is said about the superiority in this respect of reasoning by geometry above reasoning by analytical symbols, and no account is taken of analytics as an instrument of physical research. In 1833 Professor Sedgwick published "A Discourse on the Studies of the University," which in 1850 was re-published with Additions consisting of a very long Preface and Appendices, which, in fact, constitute by far the greatest part of the volume. The occasional remarks, contained in the latter publication, relative to mathematical studies, I consider to be peculiarly apposite, especially as regards what the author calls "The Newtonian System of Philosophy," and its relation to results obtained solely by experiment or observation. These views, however, refer only to Physical Astronomy. At a date somewhat later a pamphlet entitled "Remarks on the Mathematical Teaching of the University of Cambridge" was produced by Hopkins, who, by his experience both as a private tutor and as a cultivator of applied mathematics, was eminently qualified to speak on the subjects he took in hand. The pamphlet bears no date, but as it is stated on the Title-page that the author was at the time President of the British Association, it must have been circulated in 1853 or 1854. The matters it treats of are almost exclusively restricted to the *means* of teaching mathematics by private tuition, and by the Lectures of College Tutors

and University Professors, and on these points valuable suggestions are thrown out, which were afterwards to a considerable extent carried into effect. The suggestions had special reference to the Report of the Cambridge University Commission issued in 1852.

Since the last-mentioned date various changes have been made in the Scheme of Examination of Candidates for Mathematical Honours, the most important of which are those which were confirmed by Grace of the Senate on June 2, 1868, the principal effect of which was to sanction the introduction into the Examination of a wider range of questions in experimental and theoretical physics. Taking this circumstance into account, together with the limited character, as shewn by the foregoing statements, of the discussions which the Cambridge Mathematical Studies have hitherto undergone, I thought the time was come when they might with advantage be brought again under review, both as regards the principles and the reasoning adopted in the treatment of the several subjects, and as to their relation to modern advances in physical science. I may claim, I think, to have some pretensions for entering upon this undertaking, from having had to do with the study of mathematics as pupil, lecturer, examiner, or professor, during more than half a century, and having spent much time and thought on independent mathematical and physical researches.

Accordingly the contents of this publication consist

for the most part of arguments relating to the *principles* and *processes of reasoning* that are legitimate and necessary both in applied mathematics and physics, with prospective reference to the fundamental hypotheses of the Newtonian System of Philosophy, and to the methods of deducing from them by mathematical reasoning results that might be compared with modern experiments and observation. The questions considered are generally such as involve points of difficulty which, as having respect to principle, require to be cleared up before farther advances can be securely made, and the arguments employed to meet the difficulties are exhibited in as much detail as was practicable in a publication like the present; but generally the reader is referred for the details of arguments to my work on *The Principles of Mathematics and Physics* published in 1869, and to the supplementary *Essay on the Mathematical Principles of Physics*, published in 1873. I have had occasion also to cite for the same purpose communications in the *Philosophical Magazine*, especially some written after the publication of the former work. It will be proper to mention here that the first of the two above-mentioned works is uniformly cited in the Remarks, for the sake of brevity, as 'Principles' with numbers added indicating pages, and the other as 'Essay' with indications of articles.

In publishing the present work I have also had a motive of a personal kind, which I beg leave to take

this opportunity for stating. In February of next year I shall have completed the fortieth year of my tenure of the Plumian Professorship. In the ordinary course of nature I can expect to be able to continue but a short time longer the discharge of its duties, and I really think that the time is come when in respect to lecturing and examining, which are more suited to younger men, I may ask to be allowed to take the place of Professor Emeritus. I make this suggestion with the less hesitation because I feel that I am still able to devote my time to writing and publishing, and may hope thereby to contribute something both to the advancement of Natural Philosophy, and to giving a proper direction to the Mathematical Studies of this University. My plea is (1) that at the present time Natural Philosophy stands in no greater need, whether as regards the teaching of it, or its advancement, than that of being placed on the basis of the principles insisted upon by Newton in the Third Book of the Principia; and (2) that hitherto no one besides myself has undertaken to supply this need. (See what I have urged in pp. 86 and 87.) It is hoped that this brief publication may suffice to give the Authorities of the University and Members of the Senate the means of forming an opinion on these two points, and may induce them to determine, if it should be in their power, whether my being engaged in the way I propose in advancing Experimental *Philosophy* as distinct

from Experimental Physics, may be taken to be, under existing circumstances, a discharge in full of the duties of the Professorship. I certainly think that in any new arrangements that the University may make respecting the Professoriate, it would be well to provide, if practicable, that a Professor who for a long time has performed the prescribed duties of his office, should be allowed to devote himself wholly to *original research*, after giving reasonable evidence of his willingness and ability to do so.

CAMBRIDGE,
November 4, 1875.

ERRATUM.

Page 40 line 25, *for* $f(r)\frac{C_2}{r^2}$ *read* $f(r)=\frac{C_2}{r^2}$.

REMARKS,

&c.

As contributing to the main purpose of this publication, I propose to commence with giving a brief account of the Cambridge Mathematical Studies during the interval over which my acquaintance with them has extended, with reference both to the subjects taught, and to the books and lectures that have conduced to the teaching. I do not see how I can better do this, than by adducing in the first instance the following extracts from the first Report made to the University by the Board of Mathematical Studies, the date of which is May 19, 1849:

"No precise information, excepting so much as can be gathered from the published Problem Papers, can be obtained respecting the questions proposed to the Candidates for Honours before 1828, when the practice of printing all the questions was commenced. The character of the questions seems to have been determined by a few standard original works, and by the English elementary treatises in common use, which, though admirable as models of sound reasoning, certainly fell behind the progress of mathematics on the Continent. The principles of *Analytical Calculation*, published by Professor Woodhouse in 1803, and his *Plane and Spherical Trigonometry* published in 1809, are the earliest indications of the introduction of more of the *analytical* element into the mathematics of the University. But the most decided impulse in that direction was given by the translation of the *Differential and Integral Calculus* of Lacroix [8vo ed.] by Herschel, Peacock, and Babbage, in 1817,

followed by Peacock's *Examples to the Differential and Integral Calculus*, and Herschel's to the *Calculus of Finite Differences*, in 1820. The symbol of differentiation, first used in an Examination Paper in 1817, had already in 1819 superseded the symbol of the *Fluxional Calculus*. A like impulse as to the extent and character of the questions proposed in Applied Mathematics was given by the publication of Whewell's *Mechanics* (1819), Whewell's *Dynamics* (1823), Coddington's *Optics* (1823), and Woodhouse's *Plane Astronomy* (1821—1823). The first edition of Airy's *Tracts* was published in 1826, containing the Lunar Theory, the Figure of the Earth, Precession and Nutation, and the Calculus of Variations. All these subjects soon found their way into the Examination Papers. Woodhouse's *Physical Astronomy*, and his *Treatise on Isoperimetrical Problems*, had already appeared, but, not being composed in a form adapted to the University system of examinations, had not made a general impression. Analytical Geometry, which had only been read in French Treatises by the more advanced students, gained a decided footing after the publication of Hamilton's *Analytical Geometry* (1826), and Hymers' *Analytical Geometry of Three Dimensions* (1830), and now holds a prominent place in the examinations. Mr Airy, on being appointed Lucasian Professor in 1826, commenced a course of Lectures in Experimental Philosophy, which he continued (1828) as Plumian Professor. These Lectures had the effect of raising in a very important degree the standard of information among our students, and exciting their interest in classes of phenomena till then little known in the University. In 1830 Professor Airy began the practice (now fully established) of publishing the questions proposed to the candidates for Dr Smith's Mathematical Prizes. In several instances, new classes of questions, introduced for the first time into the Smith's Prize Examination, were shortly after admitted into the examinations of the candidates for Honours. Peacock's *Treatise on Algebra* (1830) had the effect of directing the attention of the students to the principles of Symbolic Calculation. The second edition of Airy's *Tracts* was published in 1831, containing, in addition

to the matter in that of 1826, the Planetary Theory, and the Undulatory Theory of Light, the latter of which had already been exemplified in his Lectures. From that time questions in each of those two subjects appear in the papers in considerable numbers. About the same time, and more recently, there occur questions in Analytical Hydrodynamics, Definite Integrals, Laplace's Coefficients, and, sparingly, in the Mathematical Theories of Electricity, Magnetism, and Heat. Very little accession of new matter has taken place in the last ten years [1839—1849], during which the character of the [Tripos] examination with respect to the subjects it includes may be asserted to have been nearly stationary."

The Report then goes on to say that "in taking the foregoing retrospect it is satisfactory to remark that the more important innovations in the subjects of examination, and in the modes of treating them, receive a kind of sanction by having originated in works written by members of our body whose names are closely connected with the scientific reputation of the country." Respecting the books that came into use, it states that "the introduction of each new subject was generally preceded by the publication of a treatise on the subject by a Cambridge mathematician, in which the Propositions were enunciated and proved in a manner suitable to the established system of examination," and that "on the different branches of Pure Mathematics and Natural Philosophy which constitute the present mathematical course, there is a considerable number of English works which are well adapted to convey exact knowledge of the subjects, and at the same time to facilitate the preparation of the students for the examinations." This, which was said in 1849, is not less applicable to the books in use at the present time.

Respecting the Lectures of the Mathematical Professors, the Report gives the following particulars. "The Lectures begun by Professor Airy twenty-three years ago, embraced Mechanics, Hydrostatics and Pneumatics, Geometrical Optics, and the Theory of Light. Lectures on the last four subjects have been continued by the present Plumian Professor since

1837, at which time also the Lowndean Professor [Dr Peacock] began courses of Lectures alternately on Astronomy and Geometry. In 1843 the Plumian Professor added Practical Astronomy to his other subjects, the Lowndean Professor having discontinued his Astronomical Lectures. The Lectures of the Plumian Professor have had the disadvantage of being given at a time when increasing activity in Astronomical Science demanded extraordinary attention to his duties at the Observatory.....Since 1838 the Jacksonian Professor has given annually a course of Lectures on Mechanism, and in the present year [1849] has added to these a series of experimental and illustrative Lectures on Mechanics." To this account it will suffice for my purpose to add here that I gave up the Lectures on Hydrostatics and Pneumatics, Geometrical Optics, and the Theory of Light, on the appointment of Professor Stokes to the Lucasian Professorship in 1849, by whom Lectures on these subjects have been given from that date to the present time. I have continued the Lectures on Practical Astronomy; and Professor Adams, who was appointed Lowndean Professor in 1858 and superintendent of the Observatory in 1861, has since the former date lectured on some branch of Physical Astronomy.

I shall now proceed to make remarks on some of the particulars of the foregoing *résumé;* but before doing so I beg to quote the following passage from a fly-sheet which I published on March 17, 1868, when the existing Regulations for the Mathematical Tripos Examination were under discussion.

"I take it for granted that the general object of the mathematical studies of the University is to exercise and strengthen the reasoning faculty, the proper use of which is a rare accomplishment. Our mathematical teaching and examinations are directed to this object, not merely in the degree that may suffice for a general education, but with the view also of training the most talented students for taking part in the advancement of mathematical and physical science. If it were not so, why should they be induced to read Physical Astronomy and the Undulatory Theory of Light? The ante-

cedents of this University and the general estimation in which science is held, especially now-a-days, alike demand that this end should be kept in view."

Accordingly the aim of the subsequent remarks is to scrutinize the principles, and the processes of reasoning, which have been adopted in the different departments of mathematics and physics, for the purpose of determining whether in the treatment of the questions there has been any admission of false principles, or any infraction of the rules of logic; and if so, to endeavour to indicate the necessary corrections.

In 1822, when I commenced the study of the Differential Calculus, the introduction of the analytical method of reasoning was, as I well remember, still regarded with jealousy by the older mathematicians of the University, from the apprehension that it was not susceptible of the rigid reasoning they considered to be characteristic of the geometrical method, especially as exemplified in Newton's *Principia*. This commendable scrupulosity was fostered by the exercises in the schools, which certainly had the good effect of practising the student in the application of rules of logic, and in the detection of flaws of reasoning. After the Fluxional Calculus had been superseded by the Differential Calculus, the mathematical publications of the time were devoted to giving as much exactness as possible to the principles of the latter. This is especially to be seen in the notes (A) and (B) appended to the beforementioned translation of the Differential and Integral Calculus of Lacroix, where the translators have quite logically derived the Differential Calculus from Lagrange's *Calculus of Functions*, this being, in fact, the only mode of derivation of that Calculus which is strictly legitimate ('Principles' p. 46). But I think that they are chargeable with some inconsistency in saying that in this manner "the Calculus may be established on principles which are entirely independent of infinitesimals or limits," inasmuch as reasoning on infinitesimals is that which essentially distinguishes the Differential Calculus from Algebraic Calculus, and these mathematicians actually do not dispense with the consideration of indefinitely small quantities

in their method of deriving the Differential Calculus. I take occasion to remark here, that the Differential Calculus when derived from the Calculus of Functions is shewn to be independent of all concrete quantity whatever, and that on this account it is more logical than the Fluxional Calculus, which is needlessly complicated by involving the idea of motion, and, by consequence, the ideas of space and time. Strict logic demands that unnecessary elements should be discarded.

The publication of Whewell's *Mechanics* and *Dynamics* (1819—1823) did away with the illogical method of proving the parallelogram of forces by means of bodies in *motion*, which had previously been adopted in English works. The proof he gave of that proposition by means of the lever, and Duchayla's proof, are both perfectly legitimate, because both rest alike on facts which constitute the fundamental principles of the Statics of rigid bodies. That given in Poisson's *Traité de Mécanique* by means of functional expressions is liable to the objection that it does not distinctly indicate what are the necessary principles of Statics. I have shewn that another proof, which may be inferred from the principle of Virtual Velocities, rests on the same fundamental facts as Whewell's and Duchayla's proofs, although this might not be readily seen to follow from the demonstration of that Principle given by Lagrange. (See the discussion of these points in 'Principles' pp. 98—104.)

Peacock in his *Treatise on Algebra* speaks of "the principle of the permanence of equivalent forms," according to which, for example, $a^m \times a^n = a^{m+n}$, whether the indices m and n be integral, fractional, or negative. Here, as in other instances which I shall have occasion to mention, the word "principle" is used to designate what is properly a "law", it being possible to arrive at a law of permanence of equivalent forms by reasoning from *à priori* principles, as is shewn in pp. 15—20 of the work above cited. All laws that are not self-evident admit of being proved by reasoning from ultimate principles; but it is not possible to arrive at a principle by any process of reasoning, because all reasoning is founded on principles

either self-evident, or accepted on independent grounds, or admitted conventionally.

In Chap. XII. of the same treatise, Peacock discusses at great length the meaning and applications of the sign $\sqrt{-1}$, and in particular the inferences that may be drawn from the expression $h(\cos\theta + \sqrt{-1}\sin\theta)$, on the supposition that it represents a straight line of length h inclined to a primitive line by the angle θ. This is in no sense the *algebraic* meaning of the expression, but on the contrary a meaning which is altogether arbitrary, or *conventional*. Let us take the case in which $\theta = \frac{\pi}{2}$, or the line is perpendicular to the primitive direction. Then the expression becomes $h\sqrt{-1}$, and is to be interpreted as indicating a line of length h perpendicular to the primitive direction. Hence it is said that the symbol $\sqrt{-1}$ expresses *perpendicularity*. But it is particularly to be noticed that it has this meaning only by reason of the above-mentioned *convention*. The proper algebraic effect of that symbol is simply to denote impossibility, as may be shewn by the following argument.

The equation of an hyperbola referred to its centre as origin, and to axes of rectangular co-ordinates along and perpendicular to its axis, being $\frac{x^2}{a^2} - \frac{y^2}{b^2} = 1$, if we put $x = 0$, we have $y = \pm b\sqrt{-1}$. Here the symbol $\sqrt{-1}$ has nothing to do with perpendicularity, but simply indicates that there is no ordinate, positive or negative, corresponding to the zero value of x. The general value of y given by the same equation is

$$\pm\left(1 - \frac{x^2}{a^2}\right)^{\frac{1}{2}} b\sqrt{-1},$$

in which $\sqrt{-1}$ only serves to shew that there are no ordinates corresponding to values of the abscissa x less than a. Moreover, as is known, the equation of the same hyperbola in *oblique* co-ordinates, referred to the same origin, is susceptible of the form $\frac{x'^2}{a'^2} - \frac{y'^2}{b'^2} = 1$; and for $x' = 0$ we have $y' = \pm b'\sqrt{-1}$,

which certainly does not mean perpendicularity, the ordinates being oblique.

The *algebraic* signification of $\sqrt{-1}$ has nothing arbitrary or conventional about it, but arises by a logical necessity out of the fundamental principles of algebraic calculation. This *necessary* signification ought to be carefully distinguished from any such *conventional* one as that I have given an instance of above. I insist on this the more from having noticed a tendency on the part of our students to confuse the two meanings, and to put them on the same footing, much to the detriment of exact appreciation of algebraic reasoning. On this account, before proceeding to other topics, I propose to discuss the origination and necessity of the symbol $\sqrt{-1}$.

It may be taken for granted that all the different forms of expressing quantity by means of letters, and all the symbols indicative of relations of quantities, and of operations performed upon them, have to be determined by abstract reasoning prior to, and independent of, any application of the formulæ to concrete quantity. I do not know of any kind of research which in the same degree demands the exercise of the faculty of pure reasoning, as that which is required for arriving at these determinations. I have gone into such researches, although scarcely with sufficient development, in those portions of the work already cited which are devoted to "The Principles of Arithmetic" and "The Principles of Algebra" ('Principles' pp. 4—37). For the details of the various arguments there employed the work itself would have to be consulted; but the arguments which bear immediately on the subject now under consideration, namely, the origin and logical necessity of the algebraic symbol of impossibility, are for the most part such as the following.

The rules of calculation, or of pure reasoning on quantity (for the terms are equivalent), are necessarily derived, in the first instance, from the principles of Arithmetic, in which quantities are directly expressed by means of numbers, and their order as to magnitude is consequently indicated. These rules, so far as they are independent of particular numbers,

might be expressed generally by putting letters, as a, b, c, &c., in place of the numbers employed for proving the rules in arbitrarily selected instances. This would be general, or universal, arithmetic. But the ultimate object of *algebraic* operations is to find out *unknown* quantities from certain given conditions by which they are connected with known quantities. This necessitates operating according to arithmetic rules on quantities the order of the magnitudes of which is not known. It was not till about the beginning of the seventeenth century that it was discovered this could be done by means of the signs + and − of addition and subtraction used according to certain *rules*. These *rules of signs* are in no respect arbitrary, but admit of being rigidly established by reasoning on the principle that they are to enable us to apply arithmetic rules of operation to quantity expressed as $a-b$, whether a be greater or less than b. It is on this principle that the rules of signs in addition, subtraction, multiplication, and division are demonstrated in pages 21—25 of the before-named work, under the head of "Principles of Pure Calculation."

The use of the signs + and − according to algebraic rule gives rise to an expression $-a$, which signifies that the real quantity a is affected with the sign of subtraction. Quantity so expressed may without logical fault be called "negative quantity," because on substituting x for an *unknown* quantity, the value of which we seek to find by the solution of an equation, there arises the necessity of admitting that x has not in all cases a real value, but may in certain cases answer the question by such negative quantity. Arithmetical rules applied to negative quantity necessitate such an expression as $\sqrt{-a}$, or $b\sqrt{-1}$, which, for a reason analogous to that just mentioned, may without logical impropriety be called "an impossible quantity," because if x be put for an unknown quantity, the value of which is to be found by the solution of an equation, it must now be admitted that the answer may be given either as a real quantity, or a negative quantity, or such impossible quantity.

The rules for the treatment of *indices* integral and fractional

can only be deduced from arithmetical considerations. But after establishing the rules of operation with arithmetical indices, we may go on to the *algebra* of indices, and by processes of reasoning analogous to those applied to ordinary algebra, prove, first, the necessity for negative indices, and then the necessity for impossible indices. In this way such expressions as $e^{\theta\sqrt{-1}}$ and $e^{-\theta\sqrt{-1}}$ may be reached antecedently to the application of algebra to geometry, or to any relations of time and space.

When the rules of signs and of the operations with negative and impossible quantities have been established on abstract and necessary principles, it is provable that every algebraic function in which numbers are put for the letters is reducible to the form $A + B\sqrt{-1}$, A and B being each either a positive or a negative numerical quantity. Hence when it is required to find an unknown quantity which shall satisfy certain conditions, and the relation between the quantity and the data of the question has been expressed by an equation, we know beforehand that the root of the equation which gives the answer to the question will take the above form. Consequently the question has no real answer positive or negative, unless in one at least of the roots the factor B be zero. Thus algebra not only gives the answer to a question whenever the answer is a real quantity positive or negative, but also is capable of directly indicating by means of the symbol $\sqrt{-1}$ that there is no such answer. Moreover in consequence of this property of indicating the impossibility of fulfilling certain proposed conditions, there exist means for determining within what limits of the conditions real answers are obtainable. Thus $\sqrt{-1}$, as a symbol of impossibility, makes algebra a complete instrument for obtaining quantitative information.

I have gone through the foregoing discussion for the purpose of introducing the following general remark, which, as respects the logic of algebra, is of much importance. Negative and impossible quantities, and the rules of operating with them, so far as they are derived from the principle that algebra is essentially a means of applying arithmetical rules of reasoning

to quantities without knowing the order of their magnitudes, make up what may be called *necessary* algebra, inasmuch as that principle is the sole and necessary foundation of Algebra. All uses of the same symbols of operation and quantity which are not referable to this fundamental principle, ought for distinction to be put under the head of *conventional* algebra.

I take occasion here to mention that after the analytical methods of reasoning had been fully adopted by Cambridge Mathematicians, there arose much speculation as to the uses and interpretation of the symbols of operation and quantity and of their combinations. For example, an abstract signification not derivable from the principles of arithmetical calculation, was attached to the signs + and −, so that in a particular instance of such speculation, these signs were reasoned upon without being attached to symbols of quantity, and expressions such as $(+)^{\frac{1}{m}}$ and $(-)^{\frac{1}{m}}$ were arrived at. Also, the conventional representation of a line in magnitude and direction was, by a kind of inverse logic, treated as a *principle* from which the rules of abstract algebraic operations were derived, and the consequent application of the symbol of impossibility to geometry, discussed in p. 7, was called "the geometrical representation of impossible quantity;" whereas, as is argued above, the symbolic representation of impossible quantity is a logical deduction from the definition of Algebra apart from all geometrical considerations whatever.

The publication of Peacock's *Algebra* in 1830, and more especially his "Report on certain Branches of Analysis" read at the Meeting of the British Association at Cambridge in 1833, seem to have had the effect of bringing conventional Algebra into greater notice. I have already (p. 7) spoken of the conventional representation, in the former work, of a straight line in magnitude and direction by the formula $h(\cos\theta + \sqrt{-1}\sin\theta)$. The report contains a variety of additional particulars and discussions relating to speculative Algebra, bringing down the history of this species of calculation to the above-mentioned date (1833). The same kind of analysis was much extended in

the year 1844 by the publication of Hamilton's *Quaternions*, and De Morgan's *Triple Algebra*. It would be beside my purpose to say anything about the applications, or the utility, of these different kinds of conventional Algebra; but there is one remark relating to them which I think it important to make. Neither in the algebraic researches referred to above, nor in any work on Algebra with which I am acquainted, have I met with any recognition of the principle or definition already enunciated, namely, that Algebra is essentially a symbolic means of applying arithmetic rules of calculation to quantities the order of the magnitudes of which is not indicated. I think I am justified in saying, that the non-recognition of this principle, as the point of departure for determining the rules of signs in algebraic operations, has given occasion to the great amount of speculation that has been bestowed on negative and impossible quantities. Instead of restricting themselves to those applications of impossible quantities which arise out of the principle of their origination, and on that account are necessary, mathematicians have invented applications based upon conventional interpretations of formulæ, and have thus created analytical processes which do not *necessarily*, or apart from limitations, conduct to true results.

In pages 21—37 of the 'Principles' I have undertaken to discuss the principles, the proofs of the rules, and the derivation of formulæ, in what I have called above "necessary Algebra," with the view of shewing *how* they are independent of all that is conventional or arbitrary. It follows, assuming the arguments to be good, that all algebraic results deduced from the principles and processes there established are necessarily true, and that any which are not so deducible are untrue. Consequently results obtained by means of conventional Algebra cannot with certainty be pronounced to be true, till they have also been arrived at by the rules and operations of necessary Algebra. Moreover, with regard to the exercise of the reasoning faculty, it is evidently more profitable to ascertain and apply what is necessary and indispensable in algebraic principles and proofs, than to employ methods of reasoning which are

founded on conventions and on that account may be dispensed with.

Pages 61—90 of the 'Principles' contain a dissertation on the Elements of Euclid, and generally on the application of the principles and rules of abstract calculation to geometry. With regard to the points there discussed, I propose to introduce here a few additional remarks bearing more expressly on the teaching of elementary geometry in the University. What is usually called "geometrical reasoning" is, in fact, reasoning on relations of space by means of special, or general, arithmetic combined with *diagrams*. After laying down the appropriate geometrical definitions, it is possible (as I have maintained in pages 70—88) to prove all the Propositions of Geometry by using numerical and literal symbols of quantity without having recourse to diagrams *for conducting the reasoning*. In the first four Books of Euclid special arithmetical relations expressed as "double," "half," &c., sometimes occur, but otherwise the reasoning is wholly conducted by diagrams. The fifth Book of Euclid is in principle *general* or *universal arithmetic*, algebra proper being excluded from it because straight lines are used as symbols of quantity instead of letters, and are taken as indicating by their lengths greater or less magnitude. In subsequent Books the reasoning is conducted according to the rules of proportions in general arithmetic, as demonstrated in Book v, as well as according to the rules of special arithmetic, and by means of diagrams.

If the definitions and axioms of Euclid be admitted, no fault can be found, so far as I can see, with the *processes of reasoning*, which, as exemplifications of logical rules, may, I think, be regarded as perfect. This is an important consideration as respects the character of the instruction given to our students, because, as they do not usually study formal logic, their knowledge of the rules of reasoning is acquired for the most part by reading Euclid; and as this advantage is gained when the reasoning is conducted by diagrams much more effectually than by employing literal symbols, the study of the Elements of Euclid is by all means to be retained, even though,

as must be conceded, in point of rigidity there is actually no difference between the Geometrical and Analytical methods of reasoning.

But, as is well known, there are questions relating to the *Definitions* and *Axioms* in Euclid which have been much discussed, and on the answers to which geometers to this day have not come to agreement. On this account I thought it might contribute something towards the solution of the difficulties to state, that after long consideration of such questions I have arrived at the conclusion, that all the diversities of views have arisen from the non-recognition, both in the Elements of Euclid, and in the discussions referred to, of the principle that all reasoning on quantity, whether abstract or concrete, rests on the perception of *ratio*, inclusive, of course, of *equality*. This is a logical necessity from which there is no escape. For this reason, in Geometry, or in any other science of concrete quantity, a definition which is distinctive of the science, and proper to be made a basis of reasoning, may legitimately contain terms expressing *equality* or *ratio*. For example, on this ground parallel lines may be defined as follows: "Lines in the same plane, which make equal angles towards the same parts with any line which meets them, are said to be parallel to each other." This definition is given in Art. 557 of Peacock's *Algebra*, and in a Note to Art. 145 he has remarked that the definition in Euclid, that "Parallel straight lines are in the same plane and being produced ever so far do not meet," requires to be supplemented by Axiom XII. This, however, is in no sense an axiom, but, as being capable of proof, is really a proposition. In the above-mentioned Note, Peacock has justly stated that if the foregoing definition of parallel lines be substituted for that in Euclid, "we should experience no difficulty in proving not merely the 12th axiom, but likewise any other proposition concerning them which is required for the purposes of Geometry," and that the definition in Euclid, treated as a proposition, "will then follow as a consequence". The truth of these assertions I have confirmed by arguments contained in 'Principles' pages 61, 62, 64, and 65. For the

foregoing reasons I am of opinion that the 12th axiom is only applied in support of a needless and inappropriate definition of parallel lines, and that consequently both the definition and the axiom should be banished from Geometry.

The following is another definition of parallel lines, framed on the same principles as that proposed above, and equally free from objection. We may say that two straight lines are parallel if every point of one is at the *same* distance from the other, the distance of a point from a straight line being defined (after Def. X of a perpendicular) to be the length of the perpendicular drawn to the straight line from the point. This definition of parallels is as legitimate as the other in respect to containing the idea of *equality*, and is equally adapted to call up in the mind the abstract idea of parallelism which we derive from external objects by means of the senses. Of the two definitions I consider the second to be preferable, because it can be extended more readily than the other in such manner as to define the parallelism of a straight line to a *plane*, and that of one plane to another. I take occasion to remark here that what we have to deal with in Geometry are *abstract conceptions*, derived from the indications of the senses, and that different definitions, so long as they bring to the mind the appropriate conception, are equally good. This may be said, for example, of the various ways in which it has been proposed to define a straight line, on which much needless discussion has been expended.

A definition of similar *rectilinear* figures is given as Def. I of Book VI, but because ratio as a fundamental idea is not admitted in Euclid, no general definition of similar figures, embracing those of which the boundaries are wholly or in part *curvilinear*, could be given. This is the reason that Definition XI of Book III is introduced to make good the logic of Prop. XXIII of that Book, although it is not really a definition, inasmuch as it is deducible by reasoning from the general definition of similar forms. (See 'Principles' p. 63.)

But the most noteworthy effect of the non-recognition of ratio as the fundamental idea of abstract calculation is to be

seen in the invention of Def. v of Book v, and the use made of it to prove propositions relating to *proportion*. That definition is, in fact, put in the place of a definition of proportion. But to shew that it is not really a definition, it suffices to state, that if the conception of ratio be admitted to be, *necessarily*, the basis of reasoning on quantity, and if proportion be consequently defined as *the equality of two ratios*, it is possible to give a demonstration of Def. v as if it were a proposition, and to prove, besides, all the Propositions of Book v which have relation to proportions. (The arguments here referred to are those, or at least such as those, given in 'Principles' pages 8—13.)

If it be objected that Def. v is necessary for proving Propositions i and xxxiii of Book vi, I reply that Prop. i is deducible by means of propositions in Book i from the Proposition that rectangles having the same altitude are to each other as their bases, and that *this* Proposition is *immediately* founded on the conception of ratio, so that no argument can be interposed between its enunciation and the perception of its truth. In like manner the Proposition that in a *given* circle angles at the centre have to each other the same ratio as the circumferences on which they stand, does not admit of intervening steps of reasoning, being perceived immediately to be a consequence of the fundamental conception of ratio (see 'Principles' p. 70), and from this self-evident proposition we may prove Prop. xxxiii without the aid of Def. v.

From the foregoing considerations I draw the general conclusion that the discussions which have arisen relative to certain points in the Elements of Euclid all turn upon not recognizing the principle that the reasoning is necessarily founded on the conceptions of equality and ratio, whether as regards abstract quantities, or the relations of space; for if that principle were adopted *ab initio*, we should be able, as is argued above, to get rid of the ordinary Definition of parallels, Axiom xii of Book i, Definition xi of Book iii, and Definition v of Book v, which are mainly the portions of Euclid respecting which, not without reason, dissatisfaction has been felt. In case the basis

of the reasoning were thus changed, it would be necessary to substitute for Book v one in which the same propositions would be proved by reasoning with letters as symbols of quantity instead of straight lines, in the manner I have given examples of in 'Principles' pages 8—14. The improvement of Geometrical Teaching, to which so much attention has recently been given, would, I think, in this way be effectually promoted, and the Elements of Euclid would not only be suitable for teaching the rules of logic, but would also become—what they are not at present—proper means of teaching Geometry logically.

Recurring now to the historical *résumé* of the Cambridge mathematical studies given in pages 1—6, I propose next to advert to some special points relating to the reasoning in the Lunar and Planetary Theories, and in the Undulatory Theory of Light. These subjects have formed a part of our mathematical system of instruction and examinations mainly in consequence of the publication of the first, second, and third Editions of Airy's Tracts in 1826, 1831, and 1842 respectively. With regard to the analytical reasoning in the two former Theories, the point to which I shall chiefly direct attention is the occurrence of non-periodic terms admitting of indefinite increase in solutions which have been obtained by reasoning conducted in strict accordance with the rules of analysis, and at the same time satisfy exactly the given differential equations. Now, according to a rule universally applicable in concrete mathematics, no strict mathematical deduction from the premises can be without significance. Hence that analytical circumstance is capable of, and must receive, interpretation. Are we to infer from it that the motion is necessarily non-periodic? This question is answered by saying that these terms of indefinite increase do, in fact, prove that the general and exact solution of the Problem of Three Bodies, if obtainable, would include conditions under which the motion is non-periodic; but because it is found that such terms can be got rid of by certain analytical processes, which at the same time lead to formulæ that satisfy the differential equations, it must be

admitted that there are particular conditions (also included in the exact solution), under which the motion is wholly periodic, inasmuch as the latter analytical circumstance must be significant as well as the other. There are various processes by which terms of indefinite increase may be removed from the general solution, all resting on the principle of introducing *arbitrarily* the condition of periodicity. For instance, Lagrange's method of the variation of parameters, which assumes that the instantaneous orbit is an ellipse, essentially rests on an hypothesis implying periodicity. I take occasion here to make the remark, that it is not correct to say that Lagrange's method gives an *à priori* proof of *the stability of the planetary system;* it rather gives the means of obtaining formulæ which apply only to a stable system, and thus of determining whether or not the planetary, or any other system the elements of which are known from observation, is actually stable. (See on the foregoing points 'Principles' pages 128—138.)

There is another question pertaining to the analytical reasoning in the Lunar and Planetary Theories, the character of which will, I think, be sufficiently understood from the following explanations. It will be assumed that the problem of the disturbance of elliptic motion by a third body whose orbit and motion are given, may be solved by integrating the differential equations, after suppressing the terms containing the disturbing forces, in order to obtain a first approximation to the values of the coordinates as functions of the time, and then, after substituting these values in the suppressed terms, integrating again, and so on. Supposing, for the sake of simplicity, the central body, and the disturbing and disturbed bodies, to be in the same plane, the above process, however far continued, will introduce only the four arbitrary constants usually designated as a, e, ϵ and ϖ. Also by the same process may be obtained a differential equation of the second order between r and t, the first integral of which is a differential equation of the first order and second degree between the same variables. The latter equation is that which is called (A) in 'Principles' page 139. Now, as is there shewn, if we suppose the arbitrary constant e to vanish,

the equation (A) gives the remarkable result that the orbit is a *circle* whatever be the disturbing force. But it is evident that this result is an impossibility, and consequently a difficulty here presents itself, from which, if no solution of it can be found, the only logical conclusion is that the mathematical theory of gravitation *fails*.

It is to be noticed that the above results are obtained before introducing into the equations the condition of *periodicity*. I have found, in fact, that the difficulty above stated, as well as the terms of indefinite increase which the same course of reasoning gives rise to, may be got rid of by introducing the condition that there shall be a *mean orbit;* which may be done as follows. In the first place, the terms in the equation (A) which involve exclusively the longitude of the disturbing body may be suppressed, because they would have as much positive as negative value, and the movements due to them would consequently disappear in the mean orbit. (On this principle, in the usual treatment of the Planetary Theory, in order "to calculate accurately the secular variations, we may neglect the variations of short period." Airy's Planetary Theory, Art. 144.) The differential equations altered on this principle then give by integration the mean orbit. It is thus found that the mean orbit is a revolving ellipse, such as Newton assumed it to be. Also if e_1 be the eccentricity of the mean orbit, the investigation leads to the equation, $e_1^2 = e_0^2 + \frac{m^2}{2}$, e_0^2 being an arbitrary constant, and m^2 the square of the ratio of the periodic times of the disturbed and disturbing bodies. ('Principles' p. 147.) The arbitrary constant e, introduced by the first integration, may be taken to be the actual eccentricity of an arbitrarily selected instantaneous ellipse, so that the difference between e_1 and e vanishes with the disturbing force. Hence we may suppose that $e_1 = e(1 + km^2)$, because if m be zero $e_1 = e$, and if e_1 be zero e vanishes whatever be m. Consequently if e and m be both small, as is the case when the three bodies are the Earth, the Moon, and the Sun, terms containing $e^2 \times m^2$ may be neglected, and thus with sufficient approximation $e_1^2 = e^2$. Hence

$e^2 = e_0^2 + \frac{m^2}{2}$. If now we suppose that $m = 0$, there would be no disturbing force, the actual and mean orbits would coincide, and as e^2 might have any arbitrary value between zero and unity, the same would be the case with respect to the arbitrary quantity e_0^2. If on the other hand e_1 the mean eccentricity be supposed to vanish, the orbit would necessarily always be a circle, we should have $e = 0$, and at the same time $e_0^2 = 0$ and $\frac{m^2}{2} = 0$, the last equation shewing that the disturbing force vanishes. These results agree with the inferences already drawn from the equation (A), and also inform us that $\frac{m^2}{2}$ *is an inferior limit to the value of* e^2. Consequently the supposition that $e = 0$ was illegitimate, and on that account led to an impossible result. In the case of the Moon, the inferior limit to the value of e_1, namely $\frac{m}{\sqrt{2}}$, happens to be very little less than the mean eccentricity of the Moon's orbit.

The argument of which the leading points are stated above is given in full detail in 'Principles' pages 138—148, but contains some logical faults which I have here taken occasion to correct. I have not met with any recognition of the analytical circumstance that by the supposition $e = 0$ the disturbing force is caused to vanish, neither have I been able to discover any other explanation of this peculiarity than that here offered. I am ready, however, to give attention to any other solution of the difficulty that might be proposed. The question thus raised as to the limits of the eccentricities of disturbed orbits is a novel and interesting one, and, as involving some delicate points of reasoning, I commend it to the consideration of mathematical students.

The Undulatory Theory of Light, on which, so far as regards principles and reasoning, I now proceed to make remarks, differs from the subjects that have hitherto been under consideration in respect to being a branch of *Physics*. As such it can be treated mathematically only on hypothetical principles. The

following discussion relates exclusively to the hypotheses on which the Theory rests, and to the mathematical reasoning whereby the observed laws of light are proved to be consequences of the hypotheses.

A theory of Light, called the Emission Theory, differing altogether from the Undulatory Theory, has, as is well known, been also proposed. In that theory, very minute corpuscles emitted from luminous bodies by forces which communicate to them the amazing velocity of nearly 200,000 miles in a second, are supposed to cause the sensation of light by impinging with this velocity on the eye of the observer. According to the other Theory, on the contrary, the passage of light from one point to another is effected, not by the transmission of matter, but by the propagation, at the above-named rate, of a *state* of matter, namely, an *undulatory* state, proper, when it reaches the eye, for acting dynamically on the optic nerves and producing thus the sensation of light. In these respects Light would be under conditions exactly analogous to those of sound, the propagation of which through space is known to take place by means of aerial undulations, travelling at a rate much exceeding the velocity of the particles of air which immediately produce the sound. Newton in his 'Queries' seems to have conceived of an action composed of both emission and undulation, but did not distinctly advocate an emission theory. Laplace, and, more expressly, Biot, treated of the laws of light according to the theory of emission, while Young upheld a theory of undulations. Sir John Herschel, after the publication of his valuable Article on Light in the Encyclopædia Metropolitana, in which he enters into full details respecting the principles of both theories, was heard to express the opinion that the emission theory, if it had received the same amount of attention from physicists as the other, might have had equal claims to being accepted. It is hard at this day to understand how the emission theory, considering the exceeding improbability of its hypotheses compared with those of the undulatory theory, should have held its ground so long. The main reason I believe to be that it was capable of being treated mathematically by

the same order of analysis as that which had been applied so successfully in physical astronomy, whereas the undulatory theory, properly so called, demands the solution of hydrodynamical problems, and consequently the application of differential equations of a higher rank, as to the number of variables involved, than those which are employed in the theory of gravitation. To this point there will be occasion to recur.

In the second edition of Airy's Tracts, published in 1831, the Undulatory Theory of Light is unreservedly accepted, although, as far as I remember, this view was not so exclusively maintained in the Experimental Lectures by which that publication was preceded. In what I propose to say on the Undulatory Theory I shall take the exhibition of it contained in Airy's Tracts (ed. of 1842) as representing sufficiently for my purpose its actual state, no important alteration or addition having been since made with respect to its principles and the mathematical reasoning by which it is supported. In the first place I make the remark, that it is a distinctive feature of the Undulatory Theory of Light, as now usually understood, that the phenomena of light are due to the oscillations of *discrete particles*. It ought, I think, on that account to be called an *oscillatory* theory, the term *undulatory* being from its etymology more properly applied to the vibratory movements of the elementary parts, in juxta-position, of a *continuous* medium, such as air or water.

Again, the so-called undulatory theory is characterized by the condition that the vibratory motion is not, as in aerial undulations, in the line of its propagation, but in a direction *transverse* to that of the propagation. This law is a direct inference from experience, and in order to account for it theoretically a special hypothesis has been adopted. It is supposed that the medium in which light is generated and transmitted has an *isotropic* constitution, that is, that it is composed of discrete particles so arranged, and in such manner reciprocally acted upon by forces emanating from them as centres, that a disturbance of the medium gives rise to motions transverse to the direction in which the effect of the disturbance is pro-

pagated. Now there is reason, I think, to say that the upholders of this hypothesis sometimes regard the isotropic constitution as belonging to the medium (called the *æther*) which is assumed to be the vehicle of the transmission of light, and sometimes take the discrete particles to be the constituents of the substance, as for instance a crystal, which is traversed by the light. The omission to distinguish in this respect between the æther and the crystal has given rise to misconception as to the reasons for making the hypothesis of an isotropic medium. As far as regards a crystal there is reason from the facts of crystallography for supposing that it may be composed of discrete atoms arranged according to special geometrical conditions. But there are no such observational grounds for attributing an isotropic constitution to the æther, so that the only evidence that can be adduced in support of this hypothesis must be drawn from a comparison of deductions from it with facts of experience. I propose, therefore, in the next place, to apply this test to the assumed isotropic constitution of the æther.

Fresnel's Theory of Double Refraction, which rests on the hypothesis of discrete particles, or centres of force, isotropically arranged, is usually referred to as establishing, by the explanation it gives of phenomena, the truth of that hypothesis. But before arriving at this conclusion, it is to be taken into account that there are certain facts which that theory fails to explain, although, if true, it ought to be capable of explaining them. For instance, as is admitted in Professor Stokes's Report on Double Refraction in the British Association Report for 1862, it does not account for the "simple law", that "one of the rays of a doubly refracting medium, if propagated in a principal plane, is subject to the ordinary law of refraction"; neither does it decidedly determine whether the transverse vibrations of a polarized ray are in the plane of polarization or perpendicular to it. Moreover it may be said that although Fresnel's equation of the wave-surface, obtained on an isotropic hypothesis, may be accepted as true, his Theory of Double Refraction, *mechanically* considered, is far from

being satisfactory. It becomes, therefore, a matter of essential importance to enquire why that theory is successful in some respects, while in others it fails. The answer I give to this enquiry is, that the hypothesis of an isotropic constitution is legitimate so far as regards the crystal, but not as regards the æther, and that the assumption of its application to the æther is the cause of the failures. To substantiate this view I proceed to adduce an argument the result of which is to shew the inapplicability of that hypothesis to the æther.

In the work before spoken of (p. 22) as giving a sufficiently complete account of the actual state of the Oscillatory Theory of Light, I see nothing to object to in the treatment of the theory on the adopted hypotheses, the difficulties of the subject being there candidly stated, and the positive conclusions arrived at being legitimately deduced from the premises. In particular I admit it to be a strict deduction from the hypotheses, that there are only *three* different kinds of light, elliptically polarized, circularly polarized, and plane-polarized; or if the two latter be regarded as particular cases of the first, that, in fact, there is but *one* kind of light. Now as matter of experience there is another kind of light, called common, or non-polarized, light, which the oscillatory theory gives no account of. To prove that the theory fails at this point it is only required to mention the circumstance that in order to account for the existence of common light it has been found necessary to make a new hypothesis, dependent in no manner on the hypotheses of that theory, but made gratuitously *pro hâc vice* (Airy's Tracts, Undulatory Theory of Optics, Art. 183). But this is a step which is forbidden by an indubitable rule of logic. According to the theory there is no such light as common light, whereas the reality of such light is established by experience. Hence the theory is directly contradicted by fact. Consequently the only course that is logically correct is to conclude either that the reasoning from the premises of the theory is erroneous, or that the premises are either wrong or defective, and then to endeavour to find out where the fault lies. If, as I am prepared to admit, no fault can be found

in the reasoning, it is necessary to conclude that the error has its origin in the hypotheses of the oscillatory theory. It avails nothing to take into consideration the modifications of the above-mentioned additional hypothesis proposed by Professor Stokes (Camb. Phil. Transactions, Vol. IX p. 414), because the rule of logic referred to above *forbids making any hypothesis whatever.*

If it be asked what is to be done under these circumstances, I should say at once that the oscillatory theory of discrete particles of the æther, being proved to be untenable, must be abandoned, and an endeavour be made to substitute another in its place. It may be asserted, as being a matter of scientific history, that the hypothesis of an æther so constituted that the law of the relation between its pressure and density is the same as that for air of given temperature, was departed from on account of difficulties arising from an inadequate knowledge of the proper mathematical treatment of the motion of fluids. Consequently it was an obvious proceeding to endeavour to rectify and complete the principles of Hydrodynamics with the view of laying the foundation of a theory of light that is strictly *undulatory.* This course I took when as Plumian Professor I commenced (in the year 1837) giving lectures in Optics, and I have ever since continued to carry on researches in the same direction, regarding the settlement of the principles and reasoning in Hydrodynamics as necessary not only for framing a physical theory of light, but also for theoretical investigations in other departments of physics. Having made these remarks I propose next to enter into details respecting the efforts I have made to complete the mathematical principles of Hydrodynamics, and to state the results arrived at, especially with reference to their applications in theories of the physical forces.

In Tom. VII of the *Journal de l'Ecole Polytechnique* Poisson has discussed the problem of the motion of a compressible fluid on the conditions that the velocity is in directions at right angles to a fixed plane and is a function of the time and the distance from the plane; and from the differential equation

applicable to this case he deduces, on the supposition that the fluid is acted upon by no extraneous force, a particular solution which exactly satisfies the received general equations of Hydrodynamics. From this solution I have obtained the absurd result that at the same distance from the plane the fluid may be at rest and in motion at the same moment (see 'Principles' pp. 193—197). According to the rules of Logic, this *reductio ad absurdum* proves either that the reasoning from the premises is erroneous, or that the premises are either faulty or defective. Now there is no ground for asserting that there is any fault in the reasoning or in the premises, and it must, therefore, be concluded that the premises are *defective.*

Poisson failed to draw this inference from his solution in consequence of his having obtained the rate of propagation of the motion by a method (previously employed by Lagrange) according to which the determination is made by assigning *arbitrary* limits to the initial disturbance. Thus a quantity not arbitrary, but depending only on the given elasticity of the fluid, was derived from arbitrary conditions. In place of this method, the principle of which is obviously wrong, I proposed, so long ago as 1829, one conducted by differentiation, the principle of which is now generally accepted. (See the solution of Proposition VIII in 'Principles' p. 189.)

The above-stated hydrodynamical problem has also been discussed by Mr Earnshaw in a paper *On the Mathematical Theory of Sound,* contained in the Philosophical Transactions for 1860, p. 133. After obtaining, by reasoning differing only in notation from that of Poisson, the before-mentioned particular solution, and applying to it the method of determining rate of propagation by differentiation, he makes the gratuitous assertion that the wave "will force its way in violation of our equations," and thus evades the above-mentioned *reductio ad absurdum* to which I was conducted by the same process of reasoning. This assertion, which may be regarded as implicitly admitting an absurdity, is in agreement with conclusions previously arrived at by Professor Stokes and the Astronomer Royal in their treatment of the same problem, and accordingly

after the publication of Mr Earnshaw's paper these two mathematicians expressed approval of his views. (See 'Principles' pp. xxxii and xxxiii.) On the contrary I have maintained, I think not without reason, that Mr Earnshaw's dictum is a virtual surrender of the principle of applying mathematics to determine the motion of a fluid, inasmuch as we have no means whatever of obtaining information respecting the motions of a fluid mass under given conditions other than by indications derived from "our equations".

I take this opportunity to urge upon my junior mathematical contemporaries, whether engaged in studying, or in advancing, pure and applied mathematics, the necessity of not admitting, or acquiescing in, the least deviation from the strict rules of logic. Mathematical reasoning, like all other reasoning, must be governed by logical rules. This, as I remember, was a matter much and profitably attended to, when acts and opponencies were kept in the Schools, the very principle of these exercises being to apply the syllogism in mathematical reasoning. I am of opinion that there has since been a falling off in that respect in the Cambridge mathematical studies, and that advances into new and higher regions of mathematics and physics have not always been made secure by strictly logical methods.

With respect to the problem mentioned above, I have inferred, as already stated, by a perfectly legitimate application of logical rules, from a solution of it obtained strictly according to the commonly received principles of Hydrodynamics, that an absurd result is arrived at, which indicates that those principles are *insufficient*, inasmuch as they cannot be shewn to be erroneous. In consequence of this conclusion the only course of enquiry that is open is to endeavour to discover in what way the principles may be supplemented. Accordingly I shall now proceed to exhibit the reasoning by which in my published works I have proposed to do this. It will be seen that this is a very critical enquiry when account is taken of the necessity, for the prosecution of theoretical physics, of determining the motions and pressures of a compressible elastic fluid. On this

account the reasoning referred to will be given here in considerable detail, and an endeavour will be made to state it with as much logical accuracy as possible.

The motion of a fluid mass is distinguished from that of a solid by the peculiarity that the relative positions of its elementary parts are susceptible of continual changes, whereas the parts of a solid continue in the same relative positions under all circumstances of the motion. In order to investigate generally and completely the motion of a fluid, it is necessary to *begin* with taking account of this distinguishing characteristic. This may be done by assuming as axiomatic that however varied and complex the motion may be, it must be subject to the abstract relations of space, if it be such as to admit of being calculated. Consequently, a surface conceived to be drawn in the fluid at a given instant so as to cut at right angles the directions of the motions of the particles through which it passes (which will be called *a surface of displacement*), must be continuous through either a finite, or an indefinitely small, extent. [For dynamical reasons two contiguous portions of the surface cannot make a finite angle with each other. See 'Principles' p. 181.] If now we represent the differential equation of this surface at any moment by $(d\psi) = 0$, the left-hand side being the complete differential with respect to space of an unknown function ψ, and if u, v, w be respectively the resolved parts, parallel to rectangular axes, of the velocity at the point xyz at the given moment, by Analytical Geometry we shall have u, v, w respectively *proportional* to the partial differential coefficients of ψ, with respect to the coordinates x, y, z. Consequently, λ being another unknown function, $(d\psi) = \frac{u}{\lambda}dx + \frac{v}{\lambda}dy + \frac{w}{\lambda}dz = 0$. This result gives some information respecting the composition of the function ψ. But in order that the law of geometrical continuity may be satisfied by the motion of the fluid at all times, we have further to introduce the condition that a surface of displacement may be drawn through *any given element in successive instants*. This condition is expressed by the equation $\delta(d\psi) = 0$, the symbol of variation δ having reference to the change of the

surface of displacement, both by the change of position of the given element and by the change of time. But since $\delta(d\psi) = (d\,.\,\delta\psi)$, it follows that $(d\,.\,\delta\psi) = 0$. Now $\delta\psi = \left(\frac{d\psi}{dt}\right)\delta t$, $\left(\frac{d\psi}{dt}\right)$ representing the complete differential coefficient of ψ with respect to space and time. Hence by substitution, the constant factor δt being omitted,

$$\left(d\,.\left(\frac{d\psi}{dt}\right)\right) = 0,$$

the brackets having the significations already indicated. This equation I have called the *first* general equation of Hydrodynamics. It expresses relations of *space* and *time* to which the motion is necessarily subject, independently of *quantity of matter* and the effects of *force*.

It is needless to give here the known investigation of the general equation which is usually, but very improperly, called the equation of continuity, this designation applying only to the *first* general equation. The other, which I call the *second* general equation, rests solely on the principle, or axiom, that, whatever be the motion, the quantity of matter remains the same, and it might, therefore, be named *the equation of constancy of mass*. It expresses generally relations of *space, time,* and *quantity of matter* to which the motion is necessarily subject, but does not include *force*.

The *third* general equation rests on D'Alembert's Principle (which is really an axiom), and expresses the mode in which the motion is subject to relations of *space, time, quantity of matter,* and *force,* whether the force be intrinsic, or external to the fluid.

The three general equations, with the equations $u = \lambda\frac{d\psi}{dx}$, $v = \lambda\frac{d\psi}{dy}$, $w = \lambda\frac{d\psi}{dz}$, and a given relation between the pressure p and density ρ, are equal in number to the seven unknown quantities ψ, λ, u, v, w, p, and ρ, and suffice, therefore, for

determining these quantities as functions of x, y, z, and t. It might be possible to deduce from the seven equations a single differential equation containing the variables ψ, x, y, z, and t, ψ being the principal variable; and this final equation ought to embrace all the laws of the motion that are independent of arbitrary conditions, and should also admit of being applied to any case of arbitrary disturbance. Now although this equation would be far too complicated for integration, or for application to specific cases of motion, it is important to remark that the *logic* of its formation is perfect, first, because it is derived from equations equal in number to the number of the unknown quantities, and, again, because the three general equations from which it is obtained rest on principles that are *axiomatic*.

It will be proper to enunciate here for future use the following general *Theorem* respecting the application of analysis to physical questions. When the fundamental principles of any department of applied mathematics have been expressed in the form of differential equations, the solutions of the equations are coextensive with the consequences of the principles, so that there is no consequence which the solved equations do not embrace, and no positive analytical result of the solutions which does not admit of a real interpretation. This Theorem is applied in the following inferences from the first general equation. (See Appendix I.)

By a first integration we have $\left(\frac{d\psi}{dt}\right) = \chi(t)$, and by a second $\psi = \chi_1(t) + c$, $\chi_1(t)$ being an arbitrary function of the time, and c an arbitrary constant independent of the time. According to the above Theorem, it is necessary that this result should receive interpretation. We may, in fact, infer from it that the function ψ, so far as it depends only on relations of space and time, is susceptible of two kinds of value, one in which, by reason of the term $\chi_1(t)$, the values vary in an arbitrary manner with the time, and the other in which, that term being supposed to be zero, the function has arbitrary values which do not vary with the time. The first of these conditions

cannot be satisfied by the surface of displacement at every arbitrarily selected position, unless the value of ψ be finally expressed as a function both of the coordinates and the time, and the other cannot be satisfied unless the value of ψ be finally expressed as a function of coordinates only. These two kinds of values of ψ correspond respectively to *unsteady* or *vibratory* motions, and to *steady* motions. The respective expressions can be obtained only by means of the second and third general equations.

There is also another inference which may be drawn from the first general equation prior to taking into consideration the way in which the fluid is put in motion. Let us enquire, as plainly it is allowable to do, what will be the consequence of supposing $udx + vdy + wdz$, or $\lambda(d\psi)$, to be an exact differential. First, it will follow that λ is a function of ψ and t. Again, if V^2 be substituted for $u^2 + v^2 + w^2$, the equation $\left(\frac{d\psi}{dt}\right) = \chi(t)$ may be put under the form

$$\frac{d\psi}{dt} + \frac{V^2}{\lambda} = \chi(t);$$

and if s be the length of a trajectory to the surfaces of displacement drawn from one of them in a given position at a given instant, we shall have $V = \lambda \frac{d\psi}{ds}$. Hence by substituting this value of V in the above equation, it will be seen, since λ is a function of ψ and t, that the equation contains only the variables ψ, s and t. Consequently its integral may be assumed to be $\psi = f(s, t)$, f representing a function of unknown form.

Having obtained this result, I shall next enquire, for reasons that will appear subsequently, what are the conditions under which the function ψ has at any time a maximum or minimum value. For this purpose we must, according to the Calculus of Variations, equate $(\delta\psi)$ to zero. In that case

$$\frac{d.f(s, t)}{ds}(\delta s) = 0.$$

Now this equation is satisfied either if

$$\frac{d \cdot f(s, t)}{ds} = 0,$$

or if $(\delta s) = 0$. But the former factor is equal to $\frac{d\psi}{ds}$, and, therefore, if it vanishes, $V = 0$. As, however, no specified case of disturbance is under consideration, no inference respecting a maximum or minimum value of ψ is derivable from a particular value of V. If, therefore, apart from arbitrary disturbances, there exists a *general* condition under which ψ is a maximum or minimum, this can only be inferred from the equation $(\delta s) = 0$. Now the interpretation of this equation is, that the length (s) of the trajectory reckoned from a given surface of displacement is the same at a given instant *for two contiguous terminal points*. But this cannot be the case, unless two contiguous trajectories, beginning, by hypothesis, in the given surface of displacement, also terminate in a common surface of displacement; and that this condition may be fulfilled it is necessary that the trajectories be *rectilinear*. Hence the result of the enqüiry is, that ψ may have a maximum or minimum value if

$$udx + vdy + wdz$$

be an exact differential and the motion be rectilinear. This definite inference from the first general equation must, according to the Theorem previously enunciated, be significant in such manner that it cannot without error be left out of account in the subsequent reasoning. It is obvious that both the above-stated conditions will be satisfied if the motion be in straight lines drawn from a centre and the velocity be a function of the distance from the centre; and also if the motion be in parallel lines perpendicular to a fixed plane and the velocity be a function of the distance from the plane. But as such motions exist only as being produced by *arbitrary* circumstances, they are not applicable to the present argument, which is prior to, and independent of, all arbitrary disturbances of the fluid. It would, however, be legitimate to assume that

the condition of rectilinearity is fulfilled by motion which takes place along a straight line by reason of *spontaneous* mutual action of the parts of the fluid, the straight line being relatively to the surrounding motion an *axis*, and that for the same motion $udx+vdy+wdz$ is an exact differential. Accordingly, our next step will be to enquire what would result from introducing these conditions.

Supposing the axis of the motion to be the axis of z, f to be a function of x and y only, and ϕ a function of z and t only, the condition that motion shall take place along that axis would be introduced by supposing that

$$(d\,.f\phi)=udx+vdy+wdz,$$

and that for points on the axis $f=1$, $\frac{df}{dx}=0$, and $\frac{df}{dy}=0$. For we should then have $u=\phi\frac{df}{dx}$, $v=\phi\frac{df}{dy}$, and $w=f\frac{d\phi}{dz}$; and since at all points of the axis the transverse motion would vanish, the motion would be wholly along the axis. Also since $f\phi$ has been substituted for ψ, we shall have at any point of the axis $\frac{d\psi}{dx}=0$, and $\frac{d\psi}{dy}=0$. Hence so far as ψ is a function of x and y, it may have a maximum or minimum value at each point of the axis, although by reason of the factor ϕ, the value might be different at different points at a given time, and might vary at a given point with the time. In this way, nevertheless, the assumed motion relative to an axis is shewn to be in accordance with the inference drawn by the previous reasoning from the first general equation, namely, that ψ has a maximum or minimum value wherever the motion is spontaneously rectilinear and such that $udx+vdy+wdz$ is an exact differential. This critical point being settled, we may proceed to apply the results in the case of a specified fluid, taking into account at the same time the second and third general equations. It will be supposed that the fluid is defined by the relation $p=a^2\rho$ between its pressure and its density.

On the suppositions that $(d\psi)=udx+vdy+wdz$ and that

the fluid is acted upon by no extraneous force, the second and third general equations conduct by a first approximation to the known linear equation

$$\frac{d^2\psi}{dt^2} = a^2\left(\frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} + \frac{d^2\psi}{dz^2}\right).$$

In order to take account of the foregoing inferences from the first general equation, we must substitute in this equation $f\phi$ for ψ, f and ϕ being functions respectively of x and y, and of z and t, as already defined. The result is

$$\frac{d^2\phi}{dt^2} = a^2\left\{\frac{d^2f}{fdx^2} + \frac{d^2f}{fdy^2} + \frac{d^2\phi}{dz^2}\right\}.$$

If we now assume that

$$\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} \pm \frac{b^2f}{a^2} = 0 \ldots\ldots\ldots\ldots\ldots\ldots (\alpha),$$

b^2 being an unknown constant, but not depending on any arbitrary initial circumstances of the motion, we shall have by combining the two equations

$$\frac{d^2\phi}{dt^2} - a^2\frac{d^2\phi}{dz^2} \pm b^2\phi = 0 \ldots\ldots\ldots\ldots\ldots\ldots (\beta).$$

Now the solutions of the equations (α) and (β) would give values of f and ϕ which accord with the suppositions that have already been made respecting the composition of these functions, and thus the legitimacy of the process which has conducted to the equations is confirmed. It is remarkable, moreover, that an expression for ϕ in a form which is not arbitrary is derivable by a unique process from the equation (β). (The investigation is given in 'Principles' pp. 203—205.) If the upper of the signs $\pm$ be taken, and e be substituted for $\frac{b^2}{4a^2}$, the result of the investigation may be put under the form

$$\phi = -\frac{m\lambda}{2\pi}\cos\frac{2\pi}{\lambda}\left\{z - at\left(1 + \frac{e\lambda^2}{\pi^2}\right)^{\frac{1}{2}} + c\right\}.$$

The lower sign would lead to a logarithmic expression for ϕ, and is thereby excluded, such an expression being analogous as

regards the present enquiry to an impossible root of an equation. Also since for a point on the axis $f=1$, we have for any such point $\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}=-\frac{b^2}{a^2}$, which equation decides that f, and by consequence $f\phi$ or ψ, has a *maximum* value on the axis. This inference gives a precise answer to the enquiry commenced in p. 31.

The equation (α) is of the same form as (β), and in like manner admits of being satisfied in a unique manner by an integral the form of which is not arbitrary. I have ascertained that this integral is

$$f=1-er^2+\frac{e^2r^4}{1^2.2^2}-\frac{e^3r^6}{1^2.2^2.3^2}+\&c.,$$

r being the distance from the axis. ('Principles' pp. 209 and 210.)

It is thus proved that the motion takes place in planes passing through the axis, and that at any point the velocity is $\phi\frac{df}{dr}$ in the direction transverse to the axis, and $f\frac{d\phi}{dz}$ in any direction parallel to the axis. Also if $1+\sigma$ be put for ρ, the density of the fluid at that point, we have, as is known, to the first approximation $\sigma=-\frac{f}{a^2}.\frac{d\phi}{dt}$. Thus the motion and pressure are symmetrically disposed about the axis, and their values to the first approximation are completely determined. As we have been arguing antecedently to any data as to the mode of putting the fluid in motion, these results must be regarded as defining *spontaneous* laws of the mutual action of the parts of the fluid; and since it is certain beforehand that such laws must be unique and definite, the fact that the above-mentioned results are of this character may be taken to be evidence of the truth of the reasoning whereby they were obtained.

The expression for $\frac{d\phi}{dz}$, which is the velocity along the axis, is

$$m\sin\frac{2\pi}{\lambda}\left\{z-at\left(1+\frac{e\lambda^2}{\pi^2}\right)^{\frac{1}{2}}+c\right\}.$$

Hence it follows that the rate of propagation has the same value, $a\left(1+\frac{e\lambda^2}{\pi^2}\right)^{\frac{1}{2}}$, at every point of an undulation, and accordingly it is not possible to fall into the before-mentioned absurdity of finding that the fluid may at a given position be at rest and have a maximum velocity at the same moment (see p. 26). The present argument has shewn that the supposed "violation" of the equations by the motion of the fluid was only a consequence of reasoning upon insufficient principles.

Since e, being equal to $\frac{b^2}{4a^2}$, is necessarily positive, the rate of propagation exceeds by a certain quantity the value a obtained by Newton. Laplace, conceiving that the excess might be due to the action of heat, attempted to decide this point by an *à priori* theory of caloric action. Poisson, in place of the *à priori* argument, supposed that at each point of the wave the effective accelerative force of the fluid is increased in a given ratio by the action of heat, and had recourse to *facts of experiment* for verification of this hypothesis. The experimental evidence is, however, unsatisfactory because it is drawn from compressions and dilatations of the air which were produced in *closed* spaces, and tells nothing as to what might take place in spaces not enclosed. By carrying on my hydrodynamical researches I ascertained that for the second and higher approximations the rate of propagation had still *quam proxime* the same value; and I also deduced, exclusively from the hydrodynamical theory, a numerical value of the rate of propagation exceeding by 17·5 feet the value given by observation ('Principles' pp. 205, 206, and 214—224; also Philosophical Magazine for July 1865, p. 47). From these results, obtainable only by completing the principles of Hydrodynamics, it would appear that little or no effect on the rate of propagation is attributable to the development and absorption of heat.

From the foregoing argument another inference may be drawn of which an important application will be made in a subsequent part of this Essay. The argument has shewn the

possibility of deducing from the hydrodynamical equations particular laws of vibration of the fluid without reference to any specified mode of putting the fluid in motion. Vibrations of this class, which for distinction may be called *primary*, must, according to the general theorem already several times cited, be taken account of in any case of vibratory motion produced in an *arbitrary* manner; that is, according to this Theorem it is necessary to regard vibrations arbitrarily produced as *composed* of primary spontaneous vibrations. As far as concerns *first* approximations the laws and character of the composite motion would depend only on the principle of the coexistence of small vibrations ('Principles' pp. 211—213). I have shewn how to extend the reasoning so far as to deduce formulæ for the composite motion from expressions for the primary vibrations including terms of the *second* order ('Principles,' Prop. XVII. pp. 230—239.)

In the foregoing discussion relative to the principles of Hydrodynamics, I have, for two reasons, gone into details of the analytical reasoning more fully than might seem to be appropriate to the character of the present publication; first, because the Theories of the Physical Forces I shall subsequently have to speak of depend essentially on the solution of hydrodynamical problems; and, again, because I had discovered that on some points, relating more especially to the first fundamental equation, the analytical reasoning, as given in my work on the Principles of Mathematics and Physics, might be exhibited in a more logical form. This I consider that I have now succeeded in doing.

There is also a general hydrodynamical theorem which is not expressly treated of in the above-mentioned work, although essential use is made of it in the hydrodynamical Theories of Galvanism and Magnetism; on which account I take occasion to bring it under notice here. The theorem may be enunciated in the following terms. "It is not possible that the motion of an unlimited fluid mass can be such as to transfer any portion of the fluid from one side of an unlimited fixed plane to the other without the transfer of an equal portion across

the plane in the opposite direction." (See Art. 11 of a communication on the action of a Galvanic Coil in the number of the Philosophical Magazine for September 1874.) This is a general law which can be accounted for theoretically as follows. (See Art. 10 of a communication on the Hydrodynamical Theory of Magnetism in the Phil. Mag. for June, 1872.) It may readily be shewn that the acceleration of a mass unlimited in three dimensions by the action of a finite pressure on a finite surface is an infinitesimal quantity of the *third* order, and consequently that such pressure produces absolutely no movement of the entire mass whether in a finite or an infinite interval of time. Hence if the mass be a fluid the displacement of any portion in the direction in which the pressure acts will immediately give rise to the displacement of an equal portion in the contrary direction. In other words, such pressures can produce no residual flow of the fluid across any plane perpendicular to the directions in which they act, and, therefore, no permanent transfer of fluid across any fixed plane whatever. To satisfy this condition the motion must either consist of vibrations whereby the particles are moved through equal spaces in opposite directions, or it must take place in *circuits,* or re-entering courses. The former condition is fulfilled by those vibratory motions which were found to result spontaneously from the mutual action of the parts of an elastic fluid the pressure of which varies as the density; and accordingly we may here perceive a reason why such motions exist independently of particular modes of disturbance. The other condition, as will afterwards appear, is exemplified in the hydrodynamical theories of galvanic and magnetic currents.

Hitherto the reasoning has proceeded on the supposition that $udx + vdy + wdz$ is an exact differential, in which case, as is known from geometrical considerations, straight lines drawn at a given instant in the directions of the motion in a given elementary particle converge to, or diverge from, focal lines, because, by hypothesis, they are normal to a continuous surface of displacement passing through the element. Hence

it follows that the element is undergoing change of form by the motion.

But we can conceive of the motion of a perfect *incompressible* fluid to be such, that its elementary parts do not change form while their relative positions are continually varying. For instance, this may be the case if the motion takes place in planes perpendicular to a fixed axis, and the velocity is some function of the distance from the axis. The fundamental property of separability of the parts of a fluid without sensible tangential resistance, allows of supposing different infinitely thin cylindrical shells of fluid to revolve uniformly about a common axis with different velocities. Under these circumstances elementary parts of the fluid of given forms continue to be of the same forms. The case will be the same if the shells be supposed to move uniformly parallel to the axis with velocities varying as some function of the distance from the axis. Both these are *steady* kinds of motion. Hence according to a general law of such motions they can coexist. (This law is investigated in 'Principles' pp. 241—243. See also Art. 24 of the Discussion of the Hydrodynamical Theory of Magnetism in the Philosophical Magazine for June 1872.) The composite motion will be *steady spiral motion*, and each elementary portion of the fluid of assigned form will continue to be of the same form. It does not appear possible to fulfil the condition of invariability of the geometrical forms of the elements in any way different from those above mentioned.

Since the supposition that $udx + vdy + wdz$ is an exact differential embraces all cases in which the elements change form, the integrability of that differential quantity by a factor will apply to all cases in which the elements are of constant form, it being, of course, assumed that, whether the elements change form or not, the directions of motion satisfy the condition of geometrical continuity, in virtue of which alone the motion admits of being calculated. The reasoning does not apply to the motions of a body consisting, like a mass of sand, of discrete and unconnected particles.

This being understood, I propose, in the next place, to

enquire, antecedently to any specified instance of motion, whether there are *spontaneous* modes of action of the parts of the fluid on each other, in the cases of motion for which $udx+vdy+wdz$ is integrable by a factor, in which cases, as argued above, the elements do not change form. Accordingly it is required, first, to obtain the equation of condition of the integrability of that differential quantity by a factor, which there are well known analytical rules for doing, and then to introduce into this equation the values of u, v, w which pertain to the general case of motion for which the forms of the elements are invariable. According to what has been already said, this general case consists of motion partly parallel to an axis, and partly in circles transverse to the axis, each component motion being a function of the distance from the axis. Hence if r be that distance, and the rectilinear and circular motions be respectively $F(r)$ and $f(r)$, we shall have

$$u=\frac{y}{r}f(r),\quad v=-\frac{x}{r}f(r),\quad w=F(r),$$

the axis of the rectangular coordinates x and y being perpendicular to the axis of the motion, and that of z coincident with it.

After introducing these values into the before-mentioned equation of condition, and obtaining the general equation of the spiral motion of the elements, I succeeded in obtaining therefrom, by an independent and unique course of reasoning, special forms of the functions $F(r)$ and $f(r)$, namely

$$F(r)=\frac{C_1}{r},\quad f(r)\,\frac{C_2}{r^4},$$

C_1 and C_2 being arbitrary constants independent of the time. These results, which determine laws of the motion that result from no arbitrary conditions and may consequently be designated as *spontaneous*, are essential to the hydrodynamical theory of galvanism. [The reasoning here referred to is given imperfectly in 'Principles' pp. 563—569, but much more completely in Arts 30—35 of the 'Theory of the action of a Galvanic Coil' in the Philosophical Magazine for September 1874]. Although the fluid has been supposed to be incompressible, the results apply, inclusively of small quantities of the *second* order,

to a medium so vastly elastic as that which is considered to be the vehicle of the transmission of light. I have applied like considerations to the case in which the axis of the motion instead of being rectilinear is a circle of given radius, and have also indicated in what way the reasoning might be extended so as to apply to an axis of any form. (See Arts. 36—41 of the communication in the Philosophical Magazine just cited.) It should here be remarked that although the investigation shews that the laws of the variation of the motions with distance from the axis are not arbitrary, the motions do not subsist except under the condition of a material solid axis, for otherwise the velocity would be infinitely great at the geometrical axis. Moreover, when the transverse circular motions are accompanied by motions parallel to the axis, it is necessary that the axis should form a complete circuit in order that the condition imposed by the general Theorem stated in page 30 may be fulfilled. According to this Theorem the instant the circuit is broken the flow parallel to the axis would cease.

After the preceding discussions, especially those relating to the principles and the analytical reasoning of Hydrodynamics, we may proceed to the consideration of the *foundations* of the mathematical treatment of the different parts of *Physical Theory*. With this object in view, I shall first mention a few particulars furnished by the history of the past condition and progress of Natural Philosophy. *Experimental Philosophy* may be said to have had its beginning with the writings of Bacon, who seems to have been the first to recognize formally the necessity of commencing the study of Nature by employing experiment for gaining a knowledge of facts and laws. Galileo obtained, by experiments on the motions of bodies acted upon by gravity at the earth's surface, the law of parabolic motion (called by Newton "Galileo's Theorem"), which was necessary for enabling Newton to *calculate* generally motions produced by the action of *variable* forces. The discovery by Newton of the processes of calculation proper for this purpose was the beginning of an entirely different department of Philosophy, which for distinction may be called *Theoretical Philosophy*. Kepler had established ex-

clusively as results of *observation*, his three famous Laws, which express relations of *space* and *time* by which the motions of the Planets about the Sun are governed. Newton, having adopted the *hypothesis* of an *attractive force* emanating from the constituent elements of bodies, and varying inversely as the square of the distances from the points of emanation, by mathematical reasoning therefrom arrived at Kelper's Laws, and thus not only gave *dynamical reasons* for the Laws, but also *pro tanto* proved the truth of the hypothetical law of gravity. The exactness of this law has since been abundantly demonstrated by means of the joint labours of observers and theoretical computers. The law could not possibly be deduced from observation alone, neither could it be proved to be a physical reality except by comparison of observational results with the results of theoretical calculation. The above-mentioned distinction between experimental and theoretical philosophy is well marked by the respective achievements of Kepler and Newton.

But Newton's "Principles of Natural Philosophy" are not restricted to those on which the calculations of Physical Astronomy are founded. It is a remarkable feature of his Third Book, which is mainly devoted to calculating the motions of the Moon, and of Planets and Comets, that at the beginning of it Rules of Philosophizing are laid down which apply to all departments of Physics. In particular, under Regula III. Newton has given, respecting the ultimate parts of bodies, certain definitions which he pronounces to be "the foundation of all philosophy." These definitions contain two ideas which demand especial notice. First, that the qualities of bodies which admit neither of increase nor decrease ("quae intendi et remitti nequeunt" are Newton's words), and which belong to all bodies that may be experimented upon, are to be regarded as qualities of all bodies; and, secondly, that the qualities of the least parts of bodies (i.e. of *atoms*) are only such as can be understood by sensation and experience relative to masses; the qualities, namely, of extension, hardness, mobility, and inertia. The same principles of physical philosophy were con-

temporaneously maintained by Locke. Newton says, moreover, at the end of the definitions that he by no means affirms gravity to be essential to bodies, and that he understands *vis inertiæ* to be an innate quality.

In the last paragraph of the Principia reference is made to "a certain very subtile spirit," pervading the grosser forms of body, to the agency of which Newton ascribes the forces of aggregation of elementary particles, electrical force, together with light and heat. This agent would now be called *the æther*. In Newton's time natural philosophy was not sufficiently advanced, either as regards experiments, or mathematics, for theoretical investigations respecting the modes of action of this medium; but his conception of the character of the medium itself is distinctly expressed in the following passage which occurs in his Letter to Boyle. "I suppose that there is diffused through all places an ætherial substance, capable of contracting or dilating, strongly elastic; in a word much like air in all respects, but far more subtile." (Horsley's Edition of Newton's Works, Vol. IV, p. 385.)

Having thus remarked upon the fundamentals of Natural Philosophy as conceived of by Newton, I proceed next to state them in the modified form in which I have repeatedly enunciated and employed them in my published physical researches, the modifications being such as might be suggested by the modern advances that have been made in mathematics and physics:—

"All visible and tangible substances consist of inert spherical atoms of constant magnitudes, and all physical force is either mode of *pressure* of the æther on the surfaces of the atoms, or reaction of the atoms at their surfaces due to the constancy of their form and magnitude. The æther is supposed to be a continuous elastic substance, of perfect fluidity, filling all space not occupied by atoms, of the same density everywhere when at rest, and when in motion varying in density always and at all points in exact proportion to variations of its pressure. Also the size of the atoms is supposed to be so small that even in the densest bodies they fill a very small portion of

a given space." (See the communication in the Philosophical Magazine for September 1874, Art. 1.)

I have ascribed to the atoms a definite and invariable form in conformity with the Newtonian rule that ultimate qualities admit neither of increase nor diminution, and because, in fact, the definition of an atom could not otherwise be employed as a basis for mathematical calculation. The spherical form has been hypothetically chosen not without reference to matter of experience, it being known that the properties of any perfectly fluid mass are in no respect altered by a change of the relative positions of its internal parts, which circumstance may well be conceived to be referable to a spherical form of the constituent atoms. Again, according to the principles of this philosophy the only admissible form of physical force is *pressure*, because this is the only kind of force that is understood by *personal sensation;* as when, for instance, the hand is pressed against a solid substance.

The qualities ascribed to the æther accord with the supposition that it is the vehicle of the propagation of light, as the air is that of the propagation of sound. If it be objected that the hypothesis of an æther of variable density is contradictory to the quality of invariability previously ascribed to the fundamentals of philosophy, the reply is that the *elasticity* of the æther is, in fact, assumed to be constant, and that the variability of its density is simply a consequence of the material atomic constitution which the æther must be conceived to have in common with visible and tangible substances. But notwithstanding this constitution the æther, like the air, may be treated *mathematically* as a continuous fluid, and, therefore, for all purposes of physical enquiry *is* continuous.

Since the foregoing hypotheses are either identical with, or legitimate extensions of, those which Newton asserted to be the foundation of Philosophy, I propose to comprehend the theories of the physical forces which I have founded upon them, under the general designation of *Newtonian Philosophy.* What especially characterizes this philosophy is, that *its hypotheses involve no assertion which is not comprehensible by the indications*

of common sensation and experience. This is their sole *regulative* principle, and inasmuch as the form in which I have enunciated them above conforms to this rule, I have no need to give any attention to objections made to the hypotheses themselves; but objections raised against any reasoning of which they form the basis I am bound to take notice of, and will do my best to answer.

It is because the hypotheses possess the character above stated that the physical theories raised upon them differ from the theories, so called, of modern and contemporary physicists, which consist, for the most part, of deductions of mathematical expressions of the *laws* of phenomena from special results of observation and experiment conjoined with *provisional* or *arbitrary* hypotheses. Such are Fourier's Theory of Heat, and Ampère's Theory of Galvanism and Magnetism. But confessedly in these researches the intrinsic nature of the forces, and the à priori *reasons* for the hypotheses, are left undetermined.

The final stage of physical investigation is reached, when explanations of phenomena, and *of their quantitative laws*, are given by means of mathematical deductions from *universal* hypotheses which satisfy the condition of being intelligible from sensation and experience. Till this is done, we can hardly be said to have arrived at *theory* properly so called. True and complete theory rests on hypotheses that are not only comprehensible, as just mentioned, but are also such as do not admit of being accounted for by ulterior hypotheses, and consequently are *ultimate* and *necessary*. This will be proved to be the distinctive quality of the system of hypotheses I have adopted, if they can be shewn to be adequate to the explanation of the nature, laws, and consequences of the operation, of all the different kinds of physical force. To demonstrate their appropriateness and adequacy for this purpose has been the express object of the various applications of mathematics to Physics with which I have now for a long time been occupied. Such demonstration, I am well aware, is absolutely necessary for establishing the truth of the Newtonian Theoretical Philosophy.

The arguments I have employed in the above-mentioned researches consist wholly of mathematical deductions from the physical hypotheses, and comparisons of the results with experimental facts. Assuming the comparisons to be strict and satisfactory, the truth of the Philosophy is the more completely established in proportion as the number of such comparisons is greater. One undoubted instance of the contradiction of a fact by a mathematical deduction from the physical hypotheses would suffice to prove that the hypotheses are partly, or wholly, erroneous. Accordingly I now propose to enquire whether, so far as available data hitherto indicate, the Newtonian Philosophy is confirmed by comparison of mathematical deductions from its hypotheses with facts of experience. For reasons that will afterwards be stated, I begin with the Theory of Light on the Undulatory Hypothesis.

Experiment has shewn that what is called the *polarization* of light is, in the first instance, a modification of *common* light of a definite character, but producible in a variety of ways as respects the relations of the primitive rays to visible and tangible substances. It must, therefore, be concluded on the undulatory hypothesis, that the modification pertains to the medium (the æther) wherein the light is generated and transmitted. According to the argument in pp. 33—35 the æther is susceptible, without respect to any particular mode of disturbance, of motion along and parallel to an *axis*, compounded with motion transverse to the axis. If now, being guided by experiment, we assume that the *sensation* of light *corresponds* to the part of the transverse motion which is very little distant from the axis, and that the longitudinal motion is neutral as regards the production of light, we can derive from the formula $a^2\sigma + f\frac{d\phi}{dt} = 0$, a very definite conception of the ætherial forces the action of which on the parts of the eye is followed by the sensation of light. As this is a matter of *sensation*, it is not possible to give an *à priori* reason why the corresponding motions should be restricted to positions near the axis as here supposed, any more than we can tell why the sensation depends also on

the motions being *vibratory*, or why a certain number of vibrations in a given time produce the sensation of a given *colour*. The actuality of the limitation is, however, proved *à posteriori* by its being a necessary condition in accounting for the phenomena of *polarization*.

Since the above defined motions, both longitudinal and transverse, have been arrived at by reasoning independently of any supposed case of disturbance (see pp. 30—36), we may draw the inference that they correspond to a *primitive* or *spontaneous* form of light, and that all other forms of light are derived from this under *arbitrary* circumstances. That spontaneous form is characterized by the motions being in planes passing through the axis, and by the law that the velocity and the condensation are both functions of the distance from the axis. This is the form of vibration corresponding to *common* light: the derived forms correspond to different species of *polarized* light. These assertions may be confirmed by reference to the equation in p. 34, by which the value of f is determined, namely,

$$\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} + \frac{b^2f}{a^2} = 0 \text{ ;} \quad \ldots\ldots\ldots\ldots\ldots\ldots (\alpha)$$

from which equation, since $a^2\sigma + f\frac{d\phi}{dt} = 0$, we get

$$\frac{d^2\sigma}{dx^2} + \frac{d^2\sigma}{dy^2} + \frac{b^2\sigma}{a^2} = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots (\gamma).$$

These equations indicate that f and σ may each be arbitrary functions of x and y for given values of z and t, and this analytical inference ought, according to the general Theorem enunciated in p. 30, to admit of and receive interpretation. At the same time we must take into account that f and σ are *originally* functions of r, and belong to common light. Hence it follows that the arbitrary values of f and σ which satisfy the above equations are only such as can be derived from their values for common light. If, for instance, the transverse motion at each point of a ray of common light be resolved into motions parallel to two fixed planes at right angles to each

other, it is found that the equations (α) and (γ) are satisfied by each set of vibrations. The fact that these equations can be so satisfied is the theoretical proof of the known resolvability of a ray of common light into two *equal* rays the transverse vibrations of which are in rectangular directions. The resolved rays are *polarized* rays of the first order. The same equations shew that each such polarized ray is resolvable into two *unequal* rays polarized in rectangular directions: and so on. Because the equations are linear with constant coefficients, it follows that the recomposition of the resolved rays of all the different orders reproduces the original non-polarized ray, as is known to be the case from experiment.

It is not required, regard being had to the purpose of this work, to pursue the theory of polarized light farther. The mathematical theory of polarization, and the distinguishing characteristics of plane-polarized, elliptically polarized, and circularly polarized light, will be found to be fully treated of, in accordance with the theoretical principles I have been endeavouring to explain, in pp. 330—338 of 'The Principles of Mathematics and Physics.' I shall, therefore, here only add the remark, that the distinction between common light and the different kinds of polarized light, which, as stated in p. 24, the *oscillatory* theory failed to indicate, is clearly exhibited by the *undulatory* theory, when, by the reasoning I have expounded, this theory is placed on the basis of correct hydrodynamical principles.

On the same principles the *composite* character of light is accounted for apart from any consideration of the way in which the æther is caused to vibrate. This follows at once from the argument contained in p. 37, which, when applied in the undulatory theory of light, necessitates the conclusion that all light as originally produced is *composite*. It is usual to attribute both the generation and the form of all the individual vibrations which constitute a beam of light to the impulses of solid vibrating particles, such as we have supposed the atoms to be. But the theory of polarization just discussed is directly opposed to this view, inasmuch as according to that theory the

forms of vibration are generated spontaneously by the mutual action of the parts of the fluid. (In this way only can *musical* sounds be accounted for when they result from the mutual conflict of different *streams* of air.) It is known from prismatic analysis that many parcels of light are composed of rays of all refrangibilities; as, for instance, the light of a wax-taper. Instead of conceiving each of these numberless sets of vibrations to be produced by the vibrations of a separate atom, we have according to our theory to regard both the composite character of the light, and the exact, or very approximate, continuity of the refrangibilities, as having an independent origin in the mutual pressures of the elementary parts of the fluid.

In the well-known experiment whereby light of definite refrangibility is obtained on putting salt into the flame of a spirit lamp, it is, I believe, admitted that the combustion generates a gas which *absorbs* rays of all refrangibilities different from that particular refrangibility. In making the experiment I have noticed, on viewing the flame through a prism at the angle of minimum deviation, that the image produced by the rays of definite refrangibility is accompanied by a very faint continuous spectrum like that of a lighted taper. This fact is in agreement with our theory of the original composite character of light, so far as regards the continuous spectrum, since it may be presumed that this faint spectrum is a residual phenomenon due to incompleteness of the above-mentioned absorption. According to the theory, all light is originally *white* light, being composed at first of rays of all refrangibilities. Consequently all instances of the generation of visible *colour* ought to be due to modifications of the primary rays by the intervention of visible and tangible substances. This theoretical inference is found to be experimentally true, it being possible to refer all cases of the sensible recognition of colour to the production, by such means, of phenomena of *Resolution, Interference,* or *Absorption.* The colours of translucent substances, whether solid, fluid, or gaseous, and the superficial colours of opaque solids, are all referable to effects of absorption. But the phenomena and laws of this kind, as

being dependent on relations of the ætherial medium to the grosser bodies, are in quite a different category from those which arise independently out of the essential qualities of the medium itself. Among the latter are to be reckoned the law of susceptibility of light to *polarization*, and the law of its *resolvability* into component parts, both of which have been specially treated of in the foregoing discussions. The first of these laws the theory ascribes to the character of the individual primary motions of the æther, as consisting of vibrations partly parallel, and partly transverse, to an axis, and the other to the total motion being originally compounded of such vibrations.

Before proceeding farther let us briefly review the argumentation as it now stands. Experiment has shewn that light is capable of resolution into components, and of being polarized, and that while these effects are producible under various arbitrary conditions, their special characters depend wholly on qualities of the medium in which light is generated and transmitted. Respecting the qualities of this medium we have admitted no other hypotheses than that it is a continuous elastic substance, perfectly fluid, indefinitely extended, and of the same density everywhere when at rest, and when in motion varying in density always and at all points in exact proportion to variations of its pressure. (See p. 43.) The Newtonian system of philosophy demands that this medium should be proved, *exclusively by mathematical reasoning founded on these premises*, to be capable of accounting for the polarization and composite character of light. This severe demand I consider that I have met by the results of the process of reasoning the principles of which are expounded in the foregoing discussion. The phenomena thus explained are so peculiar and definite, and so remote in character from the foundations on which the reasoning rests, that I might, perhaps, be justified in regarding these results as sufficient of themselves to establish the truth of the undulatory theory, and the reality of the existence of the æther. There are, however, various other explanations of phenomena of the same class, which cannot here be particularly dwelt upon; but since a bare enumeration of all such explained

phenomena will serve to strengthen the foregoing conclusion, the following list, contained in Art. 37 of the 'Essay on the Mathematical Principles of Physics,' is subjoined.

"The rectilinear transmission of light through space; its taking the form of rays; the uniformity of its rate of propagation in space; difference of intensity of different rays, and non-dependence of rate of propagation on intensity; variation of intensity according to the law of the inverse square by divergence from a centre; composite character of common light, and separability into parcels of different intensities; distinction by phase; distinction by colour; resolvability into parcels of different colours by spectrum analysis; transmutability of rays; colours resulting from compounding lights of different colours; non-interference, in general, of parcels of light from different sources; interferences under particular circumstances; distinction between common light and polarized light; resolution of common light into equal portions polarized in rectangular planes; non-dependence of the combined intensities of two oppositely polarized rays having a common path on the difference of their phases; differences, in certain proportions, of the intensities of the parts of a polarized ray resolved by a new polarization; distinctive characters of plane-polarized, elliptically-polarized, and circularly-polarized light; limited lateral divergence of rays, and phenomena of diffraction." These explanations are given in the Arts. numbered (1)—(20) under the head of THE THEORY OF LIGHT in the 'Principles of Physics.' See also Arts. 37—44 of the 'Essay.'

It should here be stated that in one instance the mathematical argument is incomplete. I refer to the limited lateral divergence of rays, on which the explanation of the phenomena of diffraction depends. The theory sufficiently indicates how limited divergence may result from the combination of an unlimited number of the vibrations of the primary type, but I have not pointed out by what process the law and the amount of the divergence are to be calculated, although I am of opinion that this might be effected by adopting the principle, employed first by Fresnel, of dividing the front of a wave into elementary

portions, and ascribing to each a limited amount of lateral divergence. According to the views previously maintained, this principle, in order that it may be legitimately applied, ought first to be deduced from the fundamental equations of Hydrodynamics. The determination I have made of the primary form of the vibrations may be considered to be a necessary step in this research. But the whole question as to the proper mode of calculating the amount of lateral divergence under given conditions is one of great difficulty, and at the same time, as I shall have occasion afterwards to shew, of great physical importance. Hitherto I have not been able to give to it the attention it demands, and can now only recommend it to the consideration of younger mathematicians. (See paragraph (20) in p. 354 of 'The Principles.')

The numerous explanations of phenomena of light which have been given, as stated above, on the hypothesis of the existence of an æther of a specific kind, may be considered to afford evidence almost amounting to certainty of the actuality of the æther, and of its being such as it was assumed to be. This is a very great step in Theoretical Physics. This position being gained, we are prepared to enter upon theoretical considerations relative to the *remaining* phenomena of light,—those which depend on its relations to visible and tangible substances, —as well as to propose a general theory of the different physical *forces*, which, according to our fundamental hypotheses, are all modes of action of the æther.

It is not necessary for my purpose to go into the details of the explanations of phenomena of light of the class mentioned above. The theory of these phenomena necessarily rests on hypotheses respecting the properties and constituency of visible and tangible substances, as well as on the mathematical consequences of the hypotheses already made relative to the æther. Hence from this class of theoretical explanations we get no additional test of the reality and supposed qualities of the *æther*, because in case of contradiction of the theory by facts the failure might be attributed to fault or defect in the hypotheses relating to the intimate condition and constituency of the sub-

stances, concerning which in the present state of physical science we have no direct means of acquiring knowledge. Experimental physics do not immediately furnish such knowledge, which ought rather to be looked for as the final result of physical enquiry pursued by all available means both experimental and theoretical. One of those means is to use the undulatory theory of light, in conjunction with data furnished by the sciences of Mineralogy and Chemistry, for acquiring information respecting the *atomic* constitution of bodies, and the *arrangement* of their atoms, as is done to a considerable extent in the undulatory theory of the Polarization and Double Refraction of rays transmitted through Crystals. These researches, it should be observed, do not require any *à priori* hypothesis to be made differing from, or additional to, those which have been proposed as the foundation of philosophy. This is an essential circumstance, inasmuch as the establishment of the Newtonian Philosophy depends on proving the *universality*, as well as the *necessity*, of the application of its hypotheses. (The theories of the class of phenomena here referred to are treated of in pp. 361—436 of 'The Principles.' See also Arts. 45 and 46 of the 'Essay.')

Again, the character of *comprehensiveness* which distinctively pertains to this system of philosophy requires that the same hypotheses should be an adequate foundation for theories of the modes of action of *all the known physical forces*. Unless this requirement can be satisfied the philosophy fails altogether. It is, therefore, necessary to ascertain whether the observed results of the action of the various physical forces, either as individual facts, or as grouped under formulated laws, can be accounted for by means of mathematical deductions from the fundamental hypotheses, conjoined with auxiliary hypotheses relating exclusively to the *arrangements*, *magnitudes*, and *numbers*, of the constituent atoms of substances. These researches, and the arguments whereby they are conducted, are of so extensive a character, that I can do no more here than refer the reader to the discussions in 'The Principles of Physics' under the several heads of the Theories of Light, Heat, Molecular Attraction,

Force of Gravity, Electric Force, Galvanic Force, and Magnetic Force. I beg also to direct attention to the abbreviated, and, in some respects, improved form in which these Theories are represented in Arts. 47—176 of the 'Essay on the Mathematical Principles of Physics,' and to the supplementary remarks and explanations in pp. 94—103 of that work. There are, however, a few additional considerations relative to these Theories, bearing more immediately on the object of the present publication, which may be suitably introduced here.

I have placed *Light* in the category of physical forces because it must be supposed that the *sensation* of light results from the dynamical action of vibrations of the æther on parts of the eye. Now it has been experimentally found that the motions communicated to these parts, so far as regards phenomena of *sight*, are caused by *transverse* vibrations of the rays. Hence since it is also known from experience that the production of light is accompanied by the production of heat, we may at once ascribe the heat to the agency of the *direct* vibrations of the same rays. This inference may, I think, be regarded as a signal instance of the adaptability of hydrodynamical theorems to account for different modes of action of physical force.

Observed effects of *Heat* (as the expansions of gases and developments of the tails of Comets) seem to indicate that heat-vibrations, by their incidence on atoms, not only put them in vibration, but are capable also of giving them accelerated motions of translation. To account for such effects simply requires, according to our theoretical principles, the solution of a very definite hydrodynamical problem, namely, to determine the motion of a free spherical atom acted upon by a series of ætherial waves. Since, however, the solution to the first approximation is found to account only for vibratory motions of the atom, it is necessary, in order to determine whether the theory accounts also for motion of translation, to meet the difficulty of proceeding to the second approximation. I made several ineffectual attempts to decide the question by this process, till at length I found that it was

only after taking into consideration the effects of lateral divergence, brought into play by the reaction of the sphere, that the second-order additional terms accounted for an accelerated motion of translation. It appeared also that according to the *greater* or *less* ratio of the excursus of a vibrating particle of the æther to the diameter of the sphere, the motion of translation might be *towards* or *from* the origin of the waves, and consequently the accelerative force be *attractive* or *repulsive*. But owing to the imperfection of the mathematical theory of lateral divergence (spoken of in p. 51), I have not attempted to ascertain precisely the respective conditions under which the direction of the acceleration is towards or from the origin. (The mathematics of this *à priori* theory of accelerative force I consider to be best given in an Article on the Hydrodynamical Theory of Attractive and Repulsive Forces contained in the Philosophical Magazine for September, 1872.)

Since, according to one of the fundamental hypotheses, "all physical force is either mode of pressure of the æther on the surfaces of atoms, or reaction of the atoms at their surfaces due to constancy of their form and magnitude," it is necessary to give reasons derived from this hypothesis for the existence and laws of *central attractive and repulsive forces*. By reasoning founded on this basis, and by making use of the foregoing general theory of accelerative forces, I have, in fact, indicated how to account generally for *atomic* or *caloric repulsion*, *molecular attraction*, *radiant heat*, and *the force of gravity*, although the mathematical reasoning employed in the investigations is in certain respects incomplete. Respecting the theories of these several forms of physical force it will now be proper to state the following particulars.

Atomic repulsion is due to the aggregate agency of those ætherial waves of the lowest order as to breadth and intensity which are generated at the surface of each atom by the reactions resulting from the incidence upon it of like waves from all surrounding atoms. The effectiveness of this force arises from the great comparative influence of lateral divergence

when waves of this class, proceeding from any atom, are incident upon a neighbouring atom. The force is *repulsive* because for this order of waves the condensations and rarefactions generated on the hemispherical surface which is turned *towards* the atom from which the waves emanate are greater than on the other half. The accelerative action of this repulsion on an atom of given size varies inversely as the *cube* of the distance of the centre of the atom from the origin of the waves, and hence, on account of the rapid increase of mutual repulsion of the atoms with diminution of the distances between them, it may be presumed that no two atoms ever come into contact.

Molecular Attraction is ascribable to composite vibrations of the *second* order, resulting from the composition of all the composite vibrations of the first order which emanate from a vast number of atoms contained within a limited spherical space. The excursus of an ætherial particle in this new order of composite vibrations is supposed to be of such extent relative to the diameter of the atom that by reason of lateral divergence an excess of condensations and rarefactions is produced on the hemispherical surface turned *from* the origin, and the atom is consequently urged by a residual force tending *towards* the origin, and therefore *attractive*. The least number of the atoms of a given substance which is required for originating vibrations that by their composition produce this attractive effect constitutes a *molecule* of the first order.

Radiant Heat may be supposed to be due to vibrations of the æther which are generally of greater breadth than those which affect the organ of sight, but have no dynamic effect corresponding to heat till their original form is broken up and transmuted by entrance into adiathermous substances. In like manner opaque, or semitransparent, substances are heated by the entrance into them of rays of light, which, being transmuted without loss of *vis viva*, give rise to heat-rays.

Fourier's "Theory of Heat" consists, as is expressly stated by its author, of mathematical deductions of laws of heat from certain fundamental facts established by experiment, and is,

therefore, avowedly an *empirical* theory. Consequently, insofar as these laws are verified by comparisons with observations, it may be demanded of the *à priori* theory of heat which I have proposed, to account for these fundamental facts. As far as I can see at present, it would be possible to meet this demand by means of the theories of atomic repulsion, molecular attraction, and radiant heat, spoken of above; but hitherto I have been prevented by occupation of my time with other physical researches from entering upon this undertaking. I may, however, state here, that I can partly see my way towards a theoretical explanation of the important *Law of exchanges*, on which so many phenomena of heat depend.

The *Force of Gravity* is referable, according to this hydrodynamical theory of the physical forces, to the action of central undulations of such an order as to breadth, and of such intensity, that the excursus of an ætherial vibrating particle exceeds very much the diameter of an atom, the force being for that reason always *attractive*. Possibly on account of the great comparative breadth of the undulations, this force may be independent of the size of the atoms, as is known to be the case when a small spherical atom is subject to the action of a *steady stream*. (See the solution of Example VIII in pp. 313—317 of 'The Principles.') The fact, however, that all bodies are equally accelerated by the force of gravity may also be accounted for by merely supposing all atoms to be of the same size. The mathematical theory is not sufficiently complete to decide whether the former of these two explanations is admissible.

A translatory effect on atoms, however small it may be, is to be attributed to the *transverse* vibrations of the rays of light, inasmuch as they are accompanied by variations of density. We may thus perceive a reason why light may be supposed capable of producing effects other than those which are recognised by the sense of sight. For instance, certain *chemical* changes which are found to be due to the action of light, might be accounted for by the translatory effect of force depending upon the *square* of the transverse velocity.

There are explanations of a different kind which the Newtonian Philosophy must be shewn to be capable of giving in order fully to demonstrate the character of universal applicability which is claimed for its hypotheses. It has been already remarked (p. 41) that Newton's method of calculating the motions of bodies acted upon by *variable* accelerative forces depends on the dynamical laws of *constant* forces which Galileo deduced from *experiments*. These laws are (1) that a constant force acting on a body in a given direction produces equal increments of velocity in that direction in equal times, independently of the actual motion, or direction of the motion, of the body; (2) that a constant accelerative force produces no motion, or change of motion, in a direction transverse to that in which it acts. (From these laws Galileo inferred the *parabolic* path of a projectile acted upon by gravity at the earth's surface.) It is, therefore, only required to give an *à priori* demonstration of these two laws in order to account completely for the mode of action of *every* kind of accelerative force. This I consider that I have done, by shewing, first, that a constant accelerative force may be attributed to the action of *a regular series of undulations*, inasmuch as such undulations can be proved to be capable of uniformly accelerating a spherical atom in the same manner and degree whatever its course and velocity may otherwise be, provided only that the velocity is small compared to the enormous velocity of propagation of undulations in the æther; and then by shewing that this action of the undulations is exclusively in the directions, normal to their fronts, in which they are propagated.

Again, the theory of *Universal Gravitation* rests on the hypotheses that each elementary portion of a mass acts upon all other elements, whether belonging to the same mass or to different masses, with a force which is directly proportional to the quantity of matter in the element, and varies inversely as the square of the distance from its centre. The truth of these hypotheses has been established by numerous comparisons of results calculated from them with facts of observation. The proposed *à priori* theory has only to account for the *hypotheses*.

It is to be understood that the elements with which we are here concerned are molecules of the *second order*, each of which, although it may be taken to be of elementary magnitude, contains a vast number of molecules of the first order, and is the origin of a series of attraction-undulations of much greater breadth than those emanating from the first-order molecules. Now the theory does, in fact, account for the action of each second-order molecule on all other molecules, inasmuch as the action is produced by undulations which are ultimately derived by composition from the primitive undulations which, as we have already argued (p. 55), emanate equally in all directions from each *atom*. The same theory evidently accounts for the law of the inverse square, this being the law of the attraction of the second-order composite undulations. The comparative feebleness of the force of gravity is due to the great breadth of the undulations, and the consequent small influence of the before-mentioned lateral divergence.

We may also see by this theory that a gravitation-molecule acts in the same manner and degree on any other molecule independently of the temperature and state of aggregation of the latter, because, as already indicated, its action on each *atom* is independent of such circumstances. But if the amount of the attraction at any distance r from the centre of an element be expressed by $\frac{\mu}{r^2}$, the same theory does not immediately explain why μ is proportional to the quantity of matter (measured by *weight*) in the element, independently of the quality, temperature, and state of aggregation, of the substance to which it belongs. It may, however, be asserted that the theoretical determination of this question would not require any addition to, or modification of, the fundamental hypotheses; and I venture to suggest that the explanation might possibly be reached by means of a theory of gravitation-exchanges analogous to the theory of heat-exchanges which, under like conditions, accounts for the *equilibrium of temperature*.

The hydrodynamical theory of physical force gives a good account of the well established *law of the mechanical equivalence*

of the force of heat, and in particular it explains how a bar of iron is made to emit both heat and light by being repeatedly struck with a hammer. (See Phil. Mag. for Oct. 1863, p. 290.)

So far we have only had under consideration physical forces the laws of which are explainable by the dynamics of *vibrations* of the æther. It will next be shewn briefly how the theories of the other forces are referable to the dynamics of the *steady motions* of the same æther.

It is a known fact that a steel bar, held in certain positions, may be permanently *magnetized* by being violently struck with a hammer at one end, and then may have its magnetism destroyed, or reversed, by being similarly struck at the other end. It is hardly conceivable that such blows can have any permanent effect, unless they produce slightly altered relative positions of the component atoms, which the intrinsic quality of steel enables it to retain. On this principle I have proposed the hypothesis that whereas in the neutral state of the bar the atomic repulsions and molecular attractions acting on a given atom are capable of separately keeping it in equilibrium, in the magnetic state the equilibrium of the atom is maintained by the counteraction of a residual attraction by a residual repulsion. I have farther shewn that under these circumstances there will be an actual, or virtual, increment of atomic density from one end of the bar to the other, and that the ætherial vibrations to which are due the residual attractions, whereby in conjunction with the residual atomic repulsions the magnetic state is maintained, will generate a *stream,* which by reason of the actual or virtual contraction of channel will be continually accelerated towards the denser end. By this action the æther, being by hypothesis an unlimited mass of inert fluid, is impressed both within the bar and in surrounding space with *steady* motion. This according to the principle enunciated in p. 38 is necessarily in re-entering courses, and on these courses, together with the variation of the pressure of the æther from point to point of space, the magnetic influence of the bar depends.

The above theory of Magnetism I have applied in account-

ing for the laws of the action of a fixed magnet on a moveable one, as determined experimentally by Gauss, (1) when the axis of the fixed magnet pointed to the middle of the other; (2) when the axis of the moveable one pointed to the middle of the fixed one, the axes being in both cases in the same horizontal plane and in rectangular directions. The theory shewed, in conformity with the results of the experiments, that the magnetic intensity varied in each case inversely as the *cube* of the distance from the middle of the fixed magnet, and for the same distance was in the former case *double* the intensity in the other. I have also ascertained that the theoretical lines of motion of the æther in the neighbourhood of a magnet agree, at least very approximately, with the directions of magnetic action determined experimentally by the Astronomer Royal by submitting a small moveable magnet to the action of a large fixed one in various positions distributed symmetrically about the latter. (See Airy's Treatise on Magnetism, p. 17.) I have not yet had time to complete the calculations required for making an exact numerical comparison of the directions thus obtained with the theory. *All* the theoretical calculations employed in the above-mentioned comparisons depend essentially on the fundamental hypothesis that the atoms are small *spheres*.

The hydrodynamical theory of Magnetism, it should be observed, gives no support to Gauss's hypothesis of two dissimilar magnetic fluids capable of separation, and so constituted that, when separated, like fluids mutually repel and unlike fluids mutually attract. It explains, however, how by means of these arbitrary hypotheses it is possible to educe from the above stated results of Gauss's experiments the law of the inverse square relative to the supposed attractions and repulsions.

After what has just been said respecting the theory of Magnetism, it will be convenient in the next place to make some remarks on the theory of *Electricity*, that term being supposed to be restricted to phenomena resulting from *friction* applied at the surfaces of bodies. In general, as was argued with respect to a magnet, the action of the molecular attractions on an atom

in the *interior* of any mass would of themselves keep it in equilibrium, or only allow of small oscillations about a mean place; and the atomic repulsions also, acting separately on the atom, would have the same effect. But this cannot be the case at and near the superficies of the substance, where, plainly, the atoms in a thin stratum must each be kept in equilibrium by the *joint* action of repulsion outwards and attraction inwards, and the atom-density of the stratum must continually diminish towards the surface. (The phenomena of *capillary attraction* are due to the sphere of molecular attraction being greater than that of atomic repulsion, and extending beyond the boundary of the substance.) Supposing now the surface of the body to be forcibly *rubbed*, the atoms of a thin superficial stratum would be disturbed, and it might be expected that they would be put into new relative positions. It is found, in fact, that by such friction the body is *electrified*. In some bodies, called *conductors*, the changed state of the superficial stratum spreads immediately over the whole of the surface, and may be entirely neutralized by a conductor which connects the surface with the earth. In others, which are *non-conductors*, the electric state extends over only a limited portion of the surface, and is more or less permanent. The difference between conductors and non-conductors depends on certain intrinsic qualities of the substances which cannot be accounted for by *à priori* theory, not because our fundamental hypotheses are inappropriate or insufficient, but because in the existing state of physical science the conditions of the problem cannot be exactly stated.

The electricity of a substance is considered theoretically to be *positive* or *negative* according as the superficial atoms are closer together, or farther apart, than in its neutral state. An enforced change of condition of the superficial stratum in either respect is accompanied by a change of the relative positions of the atoms in its interior, causing a gradual variation of atom-density analogous to that which was supposed to take place in a magnet. Taking the case of an *insulated* electrified conductor, the maintenance of the new positions of the atoms, both in the superficial stratum and in the interior, may be

conceived to be due to the action, in addition to atomic repulsions and molecular attractions, of residual undulations of a *specific* order, always coexisting with the abnormal variation of the interior atom-density due to the electric state. Undulations of this class I have called *electricity-radiants*. They are brought into sensible action by a disturbance of the neutral state of the body in respect to electricity, as heat-radiants become operative by a disturbance of the equilibrium of temperature. Also they must be supposed to traverse substances *of all kinds* (as heat-radiants traverse diathermous substances), without undergoing disruption or transmutation, partaking in this respect of the quality of gravity-radiants. Moreover it is allowable to assume that these undulations are of such an order as to breadth and intensity that they always act *attractively* on atoms, drawing them in the direction from the rarer to the denser atom-density, which, accordingly, is the direction contrary to that of the propagation of the undulations. Consequently the equilibrium of *any* atom of an electrified body results from the joint action of atomic repulsion, molecular attraction, and the attraction of electricity-radiants. According as these radiants urge the atoms at any part of the surface *towards* or *from* the interior, that part is electrified *positively* or *negatively*. (See 'Essay' Arts. 70—78.)

Electricity-radiants act not only on the atoms of the body in which they have their origin, but in like manner also on those of any body in its neighbourhood susceptible of the electric state. This is the explanation of the *induction of electricity*. It is plain that there will also be a reaction of the induced electricity on the inducing body. In short, the equilibrium of any atom of either body is maintained by atomic repulsions and molecular attractions of the body itself together with the actions of the electricity-radiants of *both*. Electric induction differs altogether from magnetic induction in respect to its operating alike on various kinds of substances, and requiring the agency of the radiants brought into play by the disturbance of the superficial atoms by *friction*. In magnetism the state of the atoms in the superficial stratum of the magnet

does not come under consideration. (See 'Principles' pp. 618—622, and 'Essay' Arts. 79—84.)

Again, the electricity-radiants of any substance which disturb by their attraction the relative positions of the superficial atoms of another substance, must be capable of moving in some degree the latter *as a whole*. No such effect, however, has been detected by experience, the reason being that if it takes place it is countervailed by the commonly observed *electrical attractions and repulsions*, which are produced by hydrodynamical action of a different kind. These forces, in fact, are due, according to our theory, to *steady motions* of the æther resulting from a gradation of atom-density in the interior of an electrified body analogous to that (already discussed in p. 60) in the interior of a magnet, although very differently produced and maintained. The steady stream is more effective than the series of undulations of the radiant, because the accelerative action in both cases depends on the *square* of the velocity of the ætherial particles, and whereas the maximum velocity in the radiant is that acquired in the short interval of a single vibration, in the stream the maximum velocity is acquired during the passage of the ætherial element over *large* spaces. Thus the theoretical explanation of mutual attractions and repulsions is very much the same for electrified bodies as for magnets. (See 'Principles' pp. 544—552 and 607, and 'Essay' Arts. 88—93, 97 and 100.)

After what has now been said on Electricity I am prepared to make some remarks on the hydrodynamical theory of *Galvanic Currents*. The conditions necessary for producing such currents will be best stated after proposing a theory of "Volta's fundamental experiment," described in Atkinson's Ganot, 2nd Edition, Art. 679. Volta proved by means of an electroscope that when a plate of copper is soldered to a plate of zinc, the copper is charged with negative electricity and the zinc with positive electricity. On our theoretical principles this effect is accounted for as follows. The two plates being called A and B, the atoms of both situated at and very near the surface of junction will be urged in one direction by molecular attractions

of A and atomic repulsions of B, and in the opposite direction by molecular attractions of B and atomic repulsions of A, and as these atoms are in equilibrium, the opposing forces on each atom must just counteract each other. This would plainly be the case if A and B were plates of the *same* metal, whether they were merely in close contact, or welded together. But if the plates be of different metals, the equilibration of the forces cannot subsist except by a rearrangement of the atoms of each of the contiguous strata, in consequence of which the atoms of one stratum are closer together, and those of the other stratum farther apart, than they are in their undisturbed state. Hence, according to the preceding theory of Electricity, one plate is positively electrified, and the other negatively electrified. This inference is confirmed by Volta's experiment, which also informs us that it is the zinc plate which is positively electrified.

By referring to the foregoing hydrodynamical theory of electricity, and taking into account the definitions of positive and negative electricities, it will be seen that under the above mentioned circumstances, there will be a tendency towards the generation of a steady stream of æther from the zinc to the copper, which, however, will not flow unless the obstacle arising from the inertia of the surrounding unlimited fluid be got rid of. This is practically done by connecting the copper plate with the zinc plate by a conducting wire (as one of copper), which completes the *circuit*, and allows of the circulation of a stream maintained in steady motion by the action at the plates. This elementary experiment involves all the conditions that are essential for generating a *galvanic current*, inasmuch as batteries, and the chemical action of acids on the plates, only have the effect of multiplying the amount of the superficial strata disturbed, and augmenting the *degree* of the disturbance.

I have explained in pp. 39—41 the course of mathematical reasoning whereby the motion of an ætherial current relatively to a fine conducting wire is ascertained. It is there shewn that the total motion is *spiral*, consisting of motion parallel

to the axis of the wire, and transverse circular motion about the same, and that the velocities vary respectively inversely as the distance, and inversely as the square of the distance, from the axis. According to this hydrodynamical theory the circular motion is necessary for the maintenance of the current, and as regards the direction of the flow the spiral motion may be either *dextrorsum* or *sinistrorsum*. But experiment indicates, in the case of a *galvanic current* along a fine *copper* wire, that the turn of the spiral is always in the *same* direction, without giving means of deciding, as far as I am able to discover, whether the direction is dextrorsum or sinistrorsum. Hence it would seem that both the circular motion and its direction are entirely due to the intrinsic molecular constitution of the copper, the effect of which on the *impressed* motion does not in the actual state of physical science admit of direct theoretical investigation.

The Numbers of the Philosophical Magazine for September, November, and December, 1874 contain Parts I., II., III. of a "Hydrodynamical Theory of the action of a Galvanic Coil on an external small Magnet." In a Postscript to Part II., I have compared angular positions of the magnet calculated from the theory with positions obtained experimentally by the Astronomer Royal. (The experiments were made under the same conditions as those referred to in p. 61, a galvanic coil being substituted for the large magnet.) The results obtained are confirmatory of the truth of the before stated mathematical theory of the galvanic current, the calculated positions of the small magnet, which entirely depend on that theory, agreeing closely with the observed positions. Farther, in the course of the discussions contained in the same Numbers of the Philosophical Magazine I have taken occasion to shew that the proposed hydrodynamical theories of magnetism and galvanism are capable of accounting for the *facts* which form the experimental basis of Ampère's *Théorie des Phénomènes Electrodynamiques uniquement déduite de l'expérience*, and I have also thereby verified, excepting as regards one of the terms, the formulæ which Ampère constructed from his experiments.

It is, at the same time, to be remarked that, as I have expressly indicated, explanations of magneto-galvanic phenomena are deducible from the *à priori* hydrodynamical theories independently of any reference to Ampère's empirical theory, as plainly should be the case, considering the fundamental character of their hypotheses. It may be demanded of the *à priori* reasoning to account for any formula, however it may have been arrived at, if it be shewn *by comparisons with experiment* to be true; but not solely because it was deduced from experiment, as the process of deduction may need confirmation. My researches give no support to the explanation of magnetic phenomena by galvanic action, although they explain why the action of a *solenoid* is approximately identical with that of a magnet.

Since, according to our theory of physical force, an ætherial current is produced wherever there is a regular gradation of atom-density in the interior of any substance, it might be expected that a lamina of metal heated *unequally* at the two ends would generate a current; and experiment shews that such is the fact. It has already been argued (p. 64) that a galvanic current is generated at the junction of two dissimilar metal plates soldered together. In Seebeck's experiment (as represented by Fig. 599 in Art. 777 of Atkinson's Ganot) the currents produced at the two junctions neutralize each other as being equal and opposite, till heat is applied to one of them, the effect of which is to make the opposite currents unequal, and thus to give rise to a residual *thermo-galvanic* current.

I have now treated of, in sufficient detail for my purpose, the theories of all the different modes of action of physical force. The foregoing discussions may, I think, be considered to give at least *primâ facie* evidence that the foundations of the Newtonian Philosophy, and the consequences deducible from them by mathematical reasoning, are as extensively capable of accounting for natural phenomena as, to be true, they are required to be. The evidence which would fully justify this conclusion is necessarily of a cumulative character; and for this reason, in the course of the discussions, I have been

careful to state any circumstances, whether as regards the mathematical reasoning or the experimental data, which might limit the applicability of the theoretical deductions, and to indicate in what directions efforts are required to be made for rendering the theories more complete. In the meanwhile it is a matter of much significance that we have advanced so far without meeting with any contradiction of the fundamental hypotheses, or any proof of their insufficiency. To these reasons for recommending mathematical physicists to direct their efforts towards the advancement of the Newtonian Philosophy, I shall here add a few other considerations, addressed more especially to Cambridge students and prosecutors of science, for the purpose of indicating the peculiar advantages they possess for qualifying themselves to undertake this kind of research.

In order to give a theoretical solution of any proposed physical question, it is required to express the conditions of the question in symbolical equations consistent with, and limited by, the fundamental hypotheses of physics, and then to deduce the consequences of the premises from solutions of the equations. For verifying a solution the mathematical results have to be compared with facts of observation. Thus the theoretical department of physics is characterized by the necessity of employing therein *à priori mathematical reasoning*. Although special applications of the reasoning require experimental data, the *reasoning* itself is prior to, and independent of, such data. For instance, Kepler's Three Laws might have been arrived at (with the aid of Galileo's laws of the action of a constant accelerative force), by mathematical reasoning founded on the hypothesis of the variation of gravity according to the law of the inverse square, if no observations, such as those of Tycho Brahe and Kepler, had ever been made. It is worth noticing that the name of Galileo occurs in the Principia of Newton no less than *six* times in the first and second Books, and each time with reference to his *dynamical* discoveries, whereas Kepler is not mentioned till his law of the sesquiplicate ratio is adduced near the beginning of Book III. under the designation of *Phenomenon*

IV. He speaks also of Kepler's "law of the Planets" in the Scholium to Prop. IV., Book III. It seems, therefore, that Galileo, as being the discoverer of *dynamical* laws, was regarded by Newton himself as his forerunner rather than Kepler, who discovered only *formal* laws. Moreover, even Galileo's laws and the law of the inverse square are derivable (as I have intimated in pp. 58 and 59) by *reasoning* from the fundamental hypotheses of physics.

But although data from experiment are not required for obtaining symbolic expressions of physical laws, this being the proper office of theoretical calculation, they are absolutely necessary for ascertaining the exact numerical values of the arbitrary quantities contained in the theoretical expressions of the laws. After these determinations have been made, the *actuality* of the laws is finally demonstrated by comparisons of numerical values calculated from the expressions with all available observations. Thus there is a necessary relation, at the same time that there is a broad distinction, between the experimental and the theoretical parts of philosophy. The reality and the data of physical science depend wholly on experiment; mathematical reasoning is necessary for making it completely *science*. Insofar as the experimenter establishes facts, and succeeds in grouping them under formulated laws, he furnishes problems the solutions of which have to be achieved by mathematical reasoning. The experiments pertaining to any branch of physics lay the foundation, and furnish the materials, of an edifice which can be completely built up only by means of mathematics. From these considerations it will be seen, since the various branches of pure and applied mathematics hold a prominent place in the studies of the Cambridge University, and are embraced to their full extent by the teaching and the publications of its graduates, that there are advantages here, scarcely to be met with elsewhere, for prosecuting the *whole* of physical science, especially since by the noble munificence of our Chancellor we have been furnished with a Physical Laboratory. Whether for the purpose of acquiring an exact knowledge of physical science, or for preparing

to make endeavours to extend its boundaries, this Institution supplies an indispensable requisite, by furnishing the means of witnessing or making the crucial and leading experiments of physics. Without acquaintance with these, such as can scarcely be acquired except by seeing them, the totality of physical science is not understood, and it would be to little purpose to attempt by original research to make advances in the theoretical part.

The *mathematics* required in the system of theoretical physics I have been advocating are not generally of a higher order, or less easily comprehended, than much that is read by the more advanced of the candidates for honours on the Mathematical Tripos, as may be seen by inspecting the contents of my work on "The Principles of Mathematics and Physics." They differ from the mathematics of ordinary dynamics in respect to the necessity, arising out of the hydrodynamical conditions of the problems, of forming and solving, sometimes to terms of the second order, differential equations containing not fewer than *three* variables. The application of such solutions, which involve not only arbitrary quantities, but also arbitrary functions, is attended with difficulties which in some instances are not yet overcome and invite farther enquiry. As there are no other means of getting answers to our interrogatories respecting the facts of nature, and their *causes*, than by forming and solving differential equations, and as the equations are more comprehensive the greater the number of variables they contain, the only way in which the advancement of theoretical physics is to be looked for is by extending and perfecting both the abstract theory of differential equations containing three or more variables, and the rules of the application of their integrals to the relations of concrete quantities.

I have had no occasion, as possibly may have been noticed, to refer to "Green's Theorem", because, comprehensive as that theorem is, it wholly depends on the conception of "force arising from attractions or repulsions whose intensity is a function of the distance only from any number of points" (Maxwell, Electricity and Magnetism, art. 70). The calculation of the results

of the combined action of such forces is really effected by ordinary differential equations of the second order, used just as in the theory of universal gravitation, although certain functions of these equations admit of being put under the form of partial differential equations, quite distinct, however, in character from those which occur in Hydrodynamics. The principal application made of such partial differential equations is to account for the pressure of a continuous medium by means of accelerative forces directed to or from discrete points. Now even if it be admitted that the pressure of a medium is explainable by this theory, it would be contrary to the principles of the Newtonian Philosophy to ascribe the pressure of the *æther* to such causation, because the existence and quality of that pressure are among the fundamental hypotheses from which all the different modes of physical force are to be deduced by mathematical reasoning. As pressure is the only kind of force immediately recognisable by the senses, it cannot, according to this philosophy, be referred to the action of any other force, but must itself account for all modes of force.

I propose to conclude these remarks by giving a precise statement of the relation in which the principles of the Newtonian Philosophy, taken as I have endeavoured to exhibit them in this Essay, stand to modern physical science and its advancement, and to infer therefrom special reasons for adopting those principles in the system of Physics taught in this University. The various means which have been employed for promoting physical science may be classed under the three heads of Experiment or Observation, Empirical Theory, and Hypothetical Theory, and the last may be subdivided into Speculative or Provisional Theory, and True Theory. Experiments and observations are made to discover and record facts, and to express their relations, as Kepler did, by formulated laws. In Empirical Theory it is also proposed to connect phenomena by formulated laws, but the formulæ are arrived at exclusively by *mathematical reasoning* based on selected normal facts or laws, the reality of which has been certified by antecedent *experience*. For this reason such theory is *empirical*. Under

this head are to be classed Fourier's Theory of Heat, Ampère's Theory of the mutual action of galvanic currents, and his theory of the action of a galvanic current on a magnetic element, which, however, is partly hypothetical. In Hypothetical Theory the object is to account for phenomena by mathematical reasoning founded on certain *hypotheses* which do not rest on any direct experimental evidence. In that case the hypotheses can be proved to be true or false only by the results of comparisons of deductions from them with the phenomena proposed to be explained. If presumptive evidence of their truth should be established by a large number of comparisons, and no contradiction be met with, the phenomena will be accounted for with a proportionate degree of certainty. The physical theories of Fresnel, Poisson, and Cauchy are of this class. The hypotheses of these theories relate for the most part to the action of forces tending to or from points, and to an isotropic constitution of media. These hypotheses, so far as they are applied to the æther, are *speculative*. Such also is Ampère's hypothesis that magnetism may be explained by the action of galvanic currents.

But the characteristic of *True* Theory is, that, while it rests of necessity on hypotheses, it admits only *such as are perfectly intelligible from sensation and experience*. It excludes, therefore, action *at a distance* without an intervening medium, because the only kind of force our senses inform us of is *pressure*, which implies *contact*. Newton adopted respecting the ultimate parts of bodies certain hypotheses intelligible from sensation and experience, which he regarded as "the foundation of all philosophy," although he has also said "hypotheses non fingo", making thus a distinction between *necessary* hypotheses, and *speculative* or *arbitrary* hypotheses. The arguments in this Essay, as in all my productions relating to theoretical physics, are directed towards shewing how to deduce by mathematical reasoning from hypotheses conforming to the above-named principle, results whereby phenomena may be explained, in order that concurrently with the explanations the truth of the hypotheses may be established.

In the theoretical department of physics the chief difficulty, as it seems to me, does not consist in selecting the appropriate hypotheses, the selection being governed, as above said, by a regulative principle, but in applying and conducting the mathematical reasoning founded on the hypotheses. On this account in the present publication, as in previous ones, I have dwelt particularly on points where the mathematics seemed to require correction or extension. To such points I beg more especially to call the attention of Cambridge mathematicians. Physical science is not established either by experiment alone, or by mathematics alone, but by a combination of both means. At the present time, in consequence of the great advances that have recently been made in experimental physics, there is greater demand for efforts being made in the theoretical than in the experimental department. As Newton was the originator of theoretical physics, this demand could no where be so appropriately met as in Newton's own University. The prestige of Newton's name calls upon the mathematicians of Cambridge to carry on what he began; and all the more because, so far as I can learn, every where else, whether in Britain, or America, or on the Continent, the principles of his theoretical philosophy are either disregarded or misunderstood. I venture now to assert, as the main conclusion to which all the antecedent arguments point, that no real progress will ever be made in the theoretical department of physics, unless in the application of mathematics the rules of logic be strictly adhered to, and unless Newton's "rules of philosophizing" be unreservedly accepted.

I should have been glad to be able to say here that my self-imposed task was completed. Since, however, I cannot but be aware that the *principles* of the Newtonian Philosophy, which I have spent so much time and labour in upholding, are opposed and rejected by advocates of a totally different system of philosophy, I feel compelled to advert to the reasons that are alleged for this opposition. In the Number of the Philosophical Magazine for July 1867 (p. 15) Professor Sir W. Thomson pronounced certain of Newton's hypotheses re-

specting the ultimate qualities of atoms to be "monstrous". Some time before this (in the Phil. Mag. for October 1863) I had argued at considerable length that the hypotheses in Newton's Third Rule of Philosophizing are, what he asserts them to be, "the foundation of all philosophy". As well, then, on my own account, as in defence of Newton's reputation as a philosopher, I shall be justified in criticising closely the grounds on which Professor Thomson expressed so strongly his dissent from Newton's principles. On examination I find that he places implicit reliance on a mathematical investigation whereby, as he judges, Helmholtz has shewn *vortex-motion* of a fluid to be of such "an absolutely unalterable character" as to justify the inference that "vortex-rings are the only true atoms". The transition from "vortex-rings" to "atoms" appears to be a *non sequitur*. Besides this, I have to make the grave objection that Helmholtz's investigation rests on imperfect hydrodynamical principles. Having obtained a solution of the problem of vortex, or spiral, motion on principles supplemented in the manner indicated in pp. 27—33, I certainly find that there is such persistence in this kind of motion, that it might be transmitted in a rectilinear course, to an indefinite distance, or for an indefinite length of time in a re-entering course, without undergoing alteration, *provided* only that the axis of the motion be coincident with the axis of a cylindrical conductor of fine wire. (See pp. 39—41.) It is by the travelling of spiral motion *along such a conductor* that galvanic signals are sent across the Atlantic. If this condition were not fulfilled the vortex-motion of the æther would come to an end like that of the *smoke-rings* with which it has been compared. The necessity of the conductor for fulfilling the condition of unalterability is fatal to the vortex-atom.

In what I have to say farther on vortex motion I shall take occasion to refer to a recently published work entitled "The Unseen Universe", so far, at least, as its contents have relation to *physics*. I do not know who are the authors of this production, but I can plainly discern that they strenuously ad-

vocate the kind of philosophy which I am now calling in question, and, therefore, if I take their account of it, I am not likely to be liable to the charge of exaggeration, or misrepresentation. In page 103 they say respecting the vortex-atom that "it yields to the least external force, and thus escapes the knife, or wriggles round it, so that it cannot be cut,—not, however, on account of its hardness, but on account of its mobility, which makes it impossible for the knife to get at it." Certainly this view accords with the literal meaning of the word "atom". But how different the vortex-atom from the atom of Newton! The latter has only such sensible qualities as are proper for constituting a *foundation* for mathematical reasoning, whereas qualities are ascribed to the other which are only recognisable by means of mathematical reasoning of a very abstruse kind, whether we regard its rotational motion, or its "wriggling" motion, the latter probably requiring complex mathematical investigation still more than the other. What will the chemist say to such an atom as this? I think I am right in entertaining the opinion that the origination of this vortex-atom is due to the overweening estimation in which Englishmen not unfrequently hold the scientific productions of *foreigners*. However this may be, the quality of the atom, and the process of reaching it, are utterly contradictory to the Newtonian principles of philosophy.

The authors of the above-mentioned work object especially to the quality of "hardness" which Newton ascribes to the least parts of bodies. The term is certainly not a good one, because it almost necessarily conveys a *relative* signification, whereas the definition of an atom must be *absolute*. For this reason I take "hardness" to mean absolute constancy of form and magnitude, regarding this quality as having been imposed by the Will and Power that brought the atom into existence, and as being maintained by the same Power. If this view be objected to, it would be as reasonable to refuse to regard *inertia* as a quality of atoms; for, like form and magnitude, this quality is perfectly intelligible from our experience of masses, is not subject to variation, nor does it depend on any ante-

cedent conditions, but must be conceived to have been originally imposed, and to be maintained, together with constancy of form and magnitude, by the will and power of the *Creator of the Universe.*

Again, these authors ask, respecting the "ultimate hard particles," what are they particles of? (p. 105.) The answer is, that they are the *constituents* of the *masses* which we see and handle, having like them the properties of size, form, mobility, and inertia. If we know what these properties are in the masses, we know what they are in the atoms, and if we don't know what they are in substances which we see and handle, we don't know *any* thing. These "least parts", as Newton calls them, may be conceived to constitute bodies under certain conditions of *arrangement*, depending on the proper atomic and molecular forces of the bodies.

As Newton attributed light, heat, electricity, and other manifestations of force, to the agency of a single elastic medium distributed through space, it is a legitimate extension of his principles to regard the pressure of this medium as the ultimate form of force. This view, however, is by no means accepted by the authors of "Unseen Universe", who seem rather to take *impact* to be the simplest form of force. I have been accustomed to regard the theorem that "impact is a short and violent pressure", as one of the best things I learnt by attending the Lectures on Mechanics given by Airy as Lucasian Professor, inasmuch as it accords with the general principle that the ultimate form of force is pressure. Exactly the reverse of this is taught by the advocates of the new phase of philosophy, who get pressure out of impact by a particular process described in pages 88 and 89 of the above-mentioned work. It will be sufficient for my purpose to advert only to the leading points of this process, as my main object is to point out the contrariety between this philosophy and that of Newton.

"Assume that a great number of small equal spherical particles of matter are enclosed in a vessel of any form, and assume further that (either by collision or by repulsive force)

each of these has the power of rebounding from another, or from the wall of the vessel, as if it were elastic, and had unit *coefficient of restitution*, as defined in treatises on natural philosophy." [I remark, by the way, that in consequence of this last condition these particles have nothing in common with the Newtonian atoms; also, that according to the argument in p. 56, no two of the latter ever come into collision.] On the above assumptions, "it can be shewn as a matter of direct calculation, that—start these particles as we please, in all sorts of directions, and with velocities as varied as we please—after a time, a sort of permanent state will be arrived at in which a certain law of distribution of velocity prevails among the particles (the same law as that of the *Probability of Error*), the greater number of them having nearly the mean square velocity....The tendency is to an average distribution of these varieties of velocity throughout the vessel, and the impacts on the sides will thus be nearly the same on every square inch of surface." Under these circumstances "*individual* particles may sometimes be moving with great, sometimes with very small, velocity," but generally with the mean square velocity. Hence as the average "energy" will be sensibly the same throughout the vessel, there is no possibility of getting work from its contents, excepting under the following conditions. "Suppose a firm partition, full of little doors (themselves without mass) to be placed so as to divide the vessel into two, and set a demon [defined to be, a conceivable finite being, having finite intelligence] at each door, with instructions to open it for an instant whenever he sees he can thereby let a quick-moving particle escape from the first compartment to the second, or a slow-moving particle from the second into the first." This process carried on long enough may be supposed "to make a considerable difference in the average velocities of the particles in the two compartments, i.e. a greater pressure per square inch on the walls of the second compartment than of the first; and thus, if the partition wall were moveable, a certain amount of work might be obtained by allowing it to move." Here I would ask, are not the above speculations to be classed with the

"somnia" of which Newton said "non confingenda sunt," and is not the necessity of calling in the aid of *intellectual* "demons", though it be only for "*guidance*", a transgression of the boundaries of *physical* science, and a fatal objection to the proposed theory of *variation of pressure?* On this point, however, I intend to say a few words in another place. (See Appendix II.)

It might be thought that I have not done justice to the authors of "Unseen Universe" in imputing to them opposition to Newton's philosophy, when account is taken of the extensive applications they have made of Newton's Third Law of Motion in pages 74—77. But on this point I have to say that my concern is exclusively with the necessary principles of *theoretical* philosophy, and that neither in the applications which these writers make of the Third Law, nor throughout their work, is there any recognition of the rule which I have so often cited as governing the hypotheses of Newton's theoretical philosophy, namely, that they must be such only as can be understood from sensation and experience. The only approach towards a recognition of this rule occurs in page 109, where they say, respecting "action at a distance", that "it is impossible (as Newton long ago pointed out in his celebrated letters to Bentley) for any one 'who has in philosophical matters a competent faculty of thinking' for a moment to admit the possibility of such action." (Faraday had already adopted the same view.) "Hence", the authors go on to say, "we have but two ways of accounting for gravitation:—either it is due to differences of pressure in a substance continuously filling all space, except where matter displaces it, or it is due to impacts."

Now, as may be gathered from what I have said in this publication and elsewhere, I have adopted the former of these ways of accounting for gravitation, and claim to have *proved mathematically* that a series of undulations of the luminiferous æther, incident upon an atom free to move, will not only cause it to vibrate, but will also communicate to it an accelerated motion of translation either in the direction of their propagation, or in the opposite direction, the æther and the atom being exactly such as they were defined to be in the fundamental

hypotheses. (Attractions caused by *vibrations* of the air have recently been experimentally recognised. See Phil. Mag. for April 1871, p. 279.) I claim also to have shewn *mathematically* that by accelerative forces due to variations of pressure in steady motions of the same medium electric and magnetic attractions and repulsions may be accounted for.

In direct contradiction to these conclusions our authors assert—but give no reasons for the assertion—that "all attempts as yet made to connect gravitation with the luminiferous æther, or the medium required to explain electric and magnetic distance-action, have completely failed; so that we are apparently driven to the impact theory as the only tenable one" (p. 109). Accordingly they give consideration to a theory of this kind proposed by Le Sage of Geneva, their account of which I subjoin, as presenting an example of the speculations which modern physicists do not scruple to put in the place of true theory.

"Le Sage assumes the existence of ultra-mundane corpuscles, in infinite numbers, even compared with those of the particles of matter; of dimensions excessively small, but flying about in all directions with velocities enormously great. Portions of gross matter virtually screen one another to a certain extent from the pressure due to this perpetual rain of corpuscles; but only on the sides turned towards one another. Hence a lone body would be equally battered on all sides; but the introduction of a second mass interferes with this arrangement, and diminishes the pressure on the side next it. It is easy to shew that the amount of this diminution, for *given small masses*, is inversely as the square of their relative distance. But when larger masses are taken account of, this diminution of pressure will not be (as gravity is) directly as the quantities of matter present, unless the farther assumption is made that matter, whether by the great distance between its particles, or by the cage-like form of these particles, is almost perfectly permeable to the corpuscles; so that, practically, the corpuscles rain upon the interior particles of a mass as freely as if each of them had been alone in space."

It is admitted that "some of the postulates of this theory are hard to grant," and the reader is referred to "Sir W. Thomson's account of it (Proc. R. S. E., 1871; and Phil. Mag. 1873 (1), p. 321), and his suggestions for its improvement, based on his theory of vortex-atoms" (p. 110). I find on reading this account that the theory postulates so much, especially as regards the actuality of "cage-atoms" and "ultra-mundane globules", that it would be better, I think, to take the action at a distance to be only a convenient provisional hypothesis, than to accept this speculative explanation of it. Notwithstanding their approval, as before mentioned, of Newton's dictum respecting such action, these physicists have evidently shewn preference for a mode of accounting for gravitation which is altogether at variance with the principles of his philosophy. Of course, from my point of view, by their refusal to regard the æther as a *continuous* and *pressing* fluid, and to treat it mathematically as such, they reject a fundamental part of natural philosophy, and are driven by the necessities of a false position to make assumptions which are *wholly imaginative*, and on that account incapable of being verified by the application of any test.

I propose next to make some remarks on the principle of "Continuity" of which so much is made in "Unseen Universe." It may certainly be gathered from the arguments in Grove's "Correlation of the physical forces" that all the forces of nature have *something* in common,—some bond of connection. Now this inference from experiments is fully accounted for by a theory which refers all the different kinds of active force to variations of pressure of one and the same medium. What mutual relations there may be between the forces, or possibilities of transmutation from one to another, can only be ascertained with certainty by mathematical reasoning founded on intelligible hypotheses relating both to the qualities of the medium, and to those of the elements of the grosser bodies on which its pressure operates. From mathematical investigations carried on during a long time in this direction, I have seen good reason for inferring the *unity* of the physical forces from their having a common origin in the pressure of the æther;

but I fail to see how the word "continuity" is applicable in any *general* sense either to the forces or to their possible transmutations. Still less do I perceive how from any principle of continuity it follows that atoms were generated from "the invisible universe." If these terms are intended to signify what is usually called "the æther," it is clear enough that such a mode of generation suits the foregone conclusion that the atom has the quality of a vortex. But according to the Newtonian doctrine there are fundamentals in philosophy which we cannot reason *up to*, but can reason *from*, and which, consequently (as I have already argued in p. 75), must be regarded as having received existence, and as continuing to exist, by the immediate will of the Creator of the Universe. In this class are included the æther, the atoms, and their respective qualities, all which, according to our principles, were created coordinately and *independently*.

As already maintained, by mathematical reasoning founded on the qualities of the æther and of atoms, we are able to account for physical phenomena. Every instance of such reasoning from these principles, if brought to a successful issue, is an approach of our intelligence to the Supreme Intelligence whereby for the ends they answer the qualities were imposed. Since from our own consciousness we know that every act implies a purpose, it is reasonable to assume that the Creator formed these elements, and made their qualities such as they are, for the express purpose of thereby enabling us to come to a knowledge of the wonderful laws by which the operations of nature are governed. For fulfilling this purpose "all things are ordered in measure, number, and weight." The knowledge of laws, thus acquired, brings us nearer to the intellectual nature of God, as the practice of moral laws brings us nearer to His moral nature. This view of the character and intent of physical laws is a complete—perhaps the only complete—answer to Hume's doctrine of the antecedent necessity of laws, and the invariable sequence of phenomena. The authors of "Unseen Universe" have unreservedly admitted the reality of creation as effected by a Personal Creator, and in consequence cannot

consistently object to interruptions of continuity which may be classed under the head of creative acts. I think, therefore, it is with some degree of inconsistency that they insist on the above mentioned principle of continuity as having determined the mode of the creation of atoms.

In the Chapter on Development the following passage occurs (p. 132):—"We think it is not so much the right or privilege as the bounden duty of the man of science to put back the direct interference of the Great First Cause—the unconditioned —as far as he possibly can in time. This is the intellectual or rather theoretical work which he is called upon to do—the post that has been assigned him in the economy of the universe." It will be seen that this "intellectual work" is different from that which I spoke of just now, which has reference to acquiring a knowledge of the operations of nature that are going on at present. With respect to the remote past I admit that it is a legitimate and interesting department of philosophy to make enquiries respecting the primordial conditions of the animal, vegetable, and material world, and (for instance) to endeavour to deduce from facts of geology and the revelations of the Telescope, what may have been the original status of the earth, and of cosmical matter generally, and what changes they may have undergone. I do not, however, see that the man of science is called upon to "put back" any thing; he has only to draw as much knowledge as he can from the use of the means of investigation he has in his power. These researches will of course be attended with great difficulties on account of the remoteness in time of the facts to be explained; but the principles on which they are to be conducted cannot be different, as far as the operations of physical force are concerned, from those that are applicable to the Physics of modern science. The probability of success in such researches will be increased in proportion as advances may be made at present in the department of *theoretical* physics.

In modern physics the word "Energy" is of very frequent occurrence, and it is therefore of importance to understand exactly what it means. In Treatises on Dynamics (as in Pratt's

Mechanical Philosophy, Arts. 496—499) a general symbolic expression of a dynamical *Law* is obtained in the form of an equation. The equation is general because it is arrived at without reference to the conditions of any particular problem, and therefore applies to all problems of ordinary dynamics; for which reason also the Law that it expresses is general. If this Law be called the Law of Vis Viva, the general equation might appropriately be called, The equation of Vis Viva. It is not logical to speak of *Principle* of Vis Viva, because the expression of the Law of Vis Viva is reached by reasoning from antecedent principles. The sole principles on which the calculation of the general equation of Vis Viva rests are the fundamental Laws of Motion, and the symbolic expression of force by the second differential coefficient of space with respect to time. It is stated in p. 75 of "Unseen Universe" that *Vis Viva* is the old name for Energy. This being the case, I now make the remark that, considering the fundamental principles on which the equation of Vis Viva, or Energy, rests, all that has been said, or can be said, about energy in ordinary dynamics, whether it be "kinetic" energy, or "potential" energy, or "the power of doing work," is simply derivable from that equation either by *general* deduction, or by assigning to the letters by which the forces are signified, and to the arbitrary constants, values appropriate to specific questions. Consequently, as far as regards ordinary dynamics, Energy (being formulated by an equation) has a determinate meaning, traceable from first principles.

But on passing from ordinary dynamics to the dynamics of Physics, or generally to dynamics requiring the application of partial differential equations, although the fundamental principles are still the same, no general equation corresponding to the formula for the Law of Vis Viva has hitherto been discovered. Consequently when Energy is spoken of relatively to the higher dynamics, it is not possible in the present state of physical science exactly to define its application, or to derive its meaning from first principles. For this reason I think it is undesirable that the word "Energy" should be put before our students; for if it means the same as Vis Viva it is not

required, and any other signification it may have is at present vague and indeterminate. Possibly the Law of the Mechanical Equivalence of Heat (spoken of in p. 59), which I have made some attempt to account for on the principles of the hydrodynamical theory of physical force by the reasoning given in 'Principles' pp. 481 and 482, may eventually be the means of conducting to a general equation of Energy applicable to the dynamics of Physics.

After what has been previously said I need not spend words in indicating how incompatible with the principles I have been maintaining are the arguments in "Unseen Universe" from which "dissipation" or "exhaustion" of energy is inferred and an estimate is formed of the possible duration of the present order of Nature. According to our principles, so long as the elasticity of the æther, and the qualities of the atoms, remain unchanged, the energy of the universe, being thence derived, must continue to be the same. It seems to me that *The Law of Exchanges* (spoken of in p. 57), which has been established experimentally with respect to heat, has an important bearing on this question. According to this Law there is every where an *equilibrium of temperature*, and a tendency to return to this state if the equilibrium be disturbed. Now since according to our principles the ætherial vibrations to which central attractions and repulsions are due, are ultimately composed of vibrations of the lowest order, which act repulsively as heat, it may be presumed that the same law of exchanges and consequent equilibrated condition pertain to all central forces, and inclusively to the force of *gravity*. Thus the gravitation of each body of the Solar System, and that of each component of the aggregate of all such systems, are uniform quantities, or, at least, are only subject to insensibly small oscillations about mean values. I take occasion to add that although within the Solar System, and far beyond, the gravitation-waves may be every where attractive, at the vast distance of the nearest fixed star from the Sun the excursus of an ætherial particle might become small compared to the diameter of an atom, in which case, as has already been supposed, the attraction would be

converted into repulsion. Consequently neighbouring stars would be mutually repulsive. This repulsion might be counteracted by an attractive order of waves produced by composition of the waves emanating from all the members of the stellar system, and thus the stability of the whole heavens would be secured. (See 'Principles' p. 505.) It is evident that the Will and the Power that created and upholds this order of the Universe might at any time put an end to it; but that this will be, or when and how it may be, *physical* science gives no means of telling.

I have now completed an ungracious task, imposed upon me by the necessity of having to find fault. It is with reluctance that I have taken up a position antagonistic to the philosophical views of contemporaries who, I have reason to think, are very eminent as physicists and mathematicians. I felt compelled to do this from having observed that the Newtonian principles of theoretical philosophy are at this time either neglected, or strenuously opposed, that true theory is in danger of being overwhelmed by speculations such as those I have had occasion to advert to in this work, and that this questionable phase of philosophy is beginning to make its way in the Cambridge system of Mathematics. The contention between me and my opponents is the old one of experiment versus theory. Flamsteed thought that the laws of the Moon's motions might be extracted from his observations apart from Newton's theoretical calculations. It was Faraday's failing to attempt to theorize without mathematics, in consequence of which he was led to entertain the idea that the force of gravitation exists only so far as it has matter to act upon. Dr Tyndall does not hesitate to maintain that *imagination* is entitled to take a part in physical research, not being aware, apparently, that although such aid may with advantage have been invoked in conducting experiments, in *theory* that is made truly such by being based on information given immediately by the *senses*, there is no place for imagination. The divergence between my philosophical views and those of physicists who agree with the authors of "Unseen Universe" has an origin of the same kind. While

I endeavour to account for experimental facts by mathematical reasoning founded, as just stated, on primordial sensible entities, they seek to make a foundation for theory out of experiment. This explains the occurrence in their writings of such terms as "work", "ergal", "reversible engine", "manufactured article", &c., used in such manner as to shew that their method of philosophy is rather an exercise of the perceptive, than of the reasoning, faculty. I regard with great admiration the ingenuity of the contrivances and fertility of resource whereby modern experimenters perform the necessary part of ascertaining facts and formulating laws (the more so, perhaps, because I am conscious that this is not my department), but they are under complete misapprehension if they expect that what may be effected by experiment can supersede the part that consists of mathematical reasoning founded on the Newtonian principles of philosophy. The old adage, *Ne sutor ultra crepidam*, applies even here. The parts are essentially distinct, but one is as necessary as the other for making up complete physical science.

The two works I have so often referred to in the course of these Remarks under the designations, 'The Principles' and 'The Essay', were composed chiefly for the purpose of bringing more into notice, and maintaining, the principles of philosophy which Newton considered to be necessary and fundamental, and extending the application of them as far as might be practicable. I am well aware that the larger work contains numerous faults and imperfections, as was to be expected in a first attempt to accomplish so extensive and complex an undertaking. If I were now to re-write that Volume I should make many corrections, and curtail much that has only personal or temporary relation to the main object, and I should take occasion to add some results of physical investigations which I have contributed to the Philosophical Magazine since the Volume was published. My object being, as I have just said, to carry out Newton's principles of theoretical philosophy, the work as to *character*, whatever may be the merits of the execution, holds the same place relatively to modern physical science as Newton's *Principia* did to the science of his day. The same ground is not

occupied by any existing Treatises on Natural Philosophy or Physics, however useful and complete they may be in other respects, and I think, therefore, that it is not without reason that I feel a desire to be released from the more immediate duties of my Professorship, in order that I might have complete leisure for preparing a new edition of the portion of this work that embraces Physics, with the view of producing a *Treatise on the Theoretical Principles of Experimental Philosophy* as exact in point of reasoning, and as free from defects, as in the existing state of physical science may be possible. I should hope by this means not only to add something to the scientific reputation of the University, but also to compose a work which, as not demanding any mathematics of a more transcendental character than the applications of partial differential equations, might be thought fit to be put into the hands of Candidates for Mathematical Honours. Remembering, however, the historical precedent furnished by the fact that the *Vortices* of Des Cartes held their ground a long time against Newton's *Principia*, I do not feel confident that this view will be assented to by my scientific contemporaries.

APPENDIX I. (See p. 30.)

THE Theorem enunciated in p. 30 relative to the applications of analysis, being perfectly general, must be true of its applications in geometry. Now there are geometrical problems in the Calculus of Variations the solutions of which, in the opinion of some mathematicians, analysis *fails* to give. Two problems in particular have been thought to exhibit instances of such failure: (1) "To determine the plane curve joining two given points which by revolving round a given axis in its plane will generate a surface of minimum area"; (2) "To determine a solid of revolution the surface of which is given, so that it may cut the axis at given points and have a maximum value". Solutions of both problems have been obtained in which the two points are connected by discontinuous or broken lines. Since, however, it is possible to draw from one point to the other different *continuous* lines giving *gradations* of values of the surface in the first problem, and *gradations* of values of the solid in the other, it is certain that there must be a particular *continuous* line corresponding to a minimum surface in the first case, and one corresponding to a maximum solid in the other.

Not being able to admit that analysis was incapable of giving the equations of these two lines (for in that case the above cited Theorem would be violated), I made various attempts, although such a research was out of my province, to obtain these equations, but without success, till at length I discovered that the difficulty might be overcome by a method of integrating differential equations involving both the principle

of integrating by a factor and that of integrating by differentiation. In the Philosophical Magazine for November 1873 I have exhibited, by means of a general theorem, the principle of that method of integrating, and the most general way of applying it. For my present purpose it will suffice to state in what manner the process has been applied in the instances of the two problems above mentioned. It is possible that a differential equation $F=0$ of the second order may have an integral containing *three* arbitrary constants, but only on the condition that the differential equation of the third order obtained by eliminating these constants from the integral is verified by the *two* equations $F=0$ and $\frac{dF}{dx}=0$, and not by either singly. The reason is that as the equation $\frac{dF}{dx}=0$ is a necessary consequence of $F=0$, the latter equation, in case the above condition is fulfilled, is really of the third order. In each of the problems $F=0$ is of the second order, and an integral containing three arbitrary constants, and satisfying the above condition, is obtained, which accordingly gives the equation of the required line. This method, however, may require additional confirmation, and if true, admits of being more extensively applied. I leave it to the consideration of younger mathematicians.

APPENDIX II. (See page 78.)

ACCORDING to views the maintenance of which has been the principal object of this publication, physical science consists essentially of two distinct parts; in one, facts are ascertained by observation and experiment, and after being recorded and grouped, are included, as far as may be, under formulated laws; in the other, *reasons* are given for the facts and laws by means of mathematical calculation founded on ultimate *hypotheses*. The selection of the hypotheses is regulated by two principles; (1) they must be such as are rendered comprehensible by antecedent information given immediately by the senses; (2) they must not be quantitative in such manner as to admit of being arrived at by mathematical reasoning, but must be appropriate foundations of such reasoning. Hence in proportion as the respective objects of the experimental and mathematical parts are successfully attained, Physics is made truly a science, and is complete in itself. Consequently any admixture of *metaphysics* with it is needless and incompatible. But although this be true there is an intimate relation between Physics and Metaphysics of such nature, that it may be questioned whether the latter has any secure scientific basis antecedently to the establishment of the other by experiment and reasoning. I have in some degree adverted to such relation in pp. 75 and 76 of the present work, and have treated of it more at length in the GENERAL CONCLUSION at the end of 'The Principles of Physics' (pp. 679—687). I think I have there said almost as much as there is need for a physicist to say on this subject. I propose,

however, to make here a few additional remarks upon it, on account of their having been suggested by reading the contents of "Unseen Universe".

Since the law according to which variations of the pressure of air of given temperature are in exact proportion to variations of its density is due, according to our principles, to the dynamical action of the æther, the pressure of which has been assumed to vary with its density according to the same law, it might be argued that the law for the æther is due to the operation of another æther of still greater tenuity, and having the same relation between its pressure and density; and so on, *ad libitum*. It may be noticed that this conception of successive æthers is very analogous to the idea of "invisible universes" of successive orders proposed in pp. 170—172 of "Unseen Universe". In my views, however, there is nothing analogous to the *generation* of atoms out of the æther or first invisible universe, of this out of the next, and so on. Assuming this succession of æthers to be a reality, it would suffice, I think, to regard them as existing and having their qualities by the immediate will of the Creator.

But must we conclude that these successive æthers are as actually existent as the æther which we consider to be the medium of the transmission of light? I think not. This conception of succession of æthers may only be the outcome of our finite intelligence when we trace the operations of nature as far as our powers of enquiry allow us to go. In the same way we are obliged to think of time, and space, and the æther, as extended *ad libitum*. Now since this mode of thinking of time and space as indefinitely extended does not affect our cognition of, and actual relations to, time and space, so neither does the conception of an indefinitely extended æther, and of an indefinite succession of æthers, affect in any way our cognition of the one æther whose existence we are assured of by *phenomena*. In short it appears to me that the only legitimate inference to be drawn from our capability of forming such *ad libitum* conceptions respecting time, space, and the æther, is, that we thus learn that these entities exist, and are such as they are, by a *Supreme Will*.

Inasmuch as according to our philosophy pressure is the only form of physical force, because it is the only kind of force we can recognize by *conscious agency*, it might be contended that on this principle there must be the consciousness of an agent in every operation of nature. I confess that I am unable to resist this conclusion, being of opinion that it is inconceivable that there can be any thing done—any *fact*—apart from the will and purpose of an agent. It was legitimate to assume the existence of an æther having the properties of pressing, and of pressing proportionately to its density, provided it could be shewn that these hypotheses were necessary for giving reasons for natural phenomena. Let it be assumed that this has been shewn, or may be shewn, by our system of philosophy, and that thus the pressure and variation of pressure of the æther may be proved to be realities. The question may then be asked, What account can be given of the agency concerned in this pressure and variability of pressure? It is to be noticed that this is not a *physical* question; we have now passed the boundary of physics. It can, therefore, receive answer only by reference to *metaphysical*, or *spiritual*, agency. My principles, consequently, do not oblige me to object *in toto* to the "demons of finite intelligence" introduced, as mentioned in p. 77, to account by their agency for variation of pressure, but I object to their being introduced *in the department of physics*. (Also I would give them the name of "angels", or "ministers", rather than call them "demons".) It seems to me not unreasonable to suppose, since we, as partaking of a spiritual nature, are endowed with power over the gross matter which constitutes our bodies, to move it at our will within prescribed limits, that there may be intelligent spiritual beings of another order by whose conscious and immediate agency, exerted in fulfilment of their Maker's will, the pressure of the æther, and the law of variation of its pressure, are maintained, that thereby it may perform its destined physical functions. This view is in conformity with the teaching of the Scriptures respecting angels, to whose agency they uniformly ascribe what we call Nature's operations, apparently because, as

I have already said, it is inconceivable that there can be any production, or event, apart from the purpose and consciousness of an operator.

After what has just been said I think it right to advert briefly to the parts of "Unseen Universe" in which physical science is brought into connection with some of the deepest mysteries of the Christian Religion. I am far from being one of those who object altogether to taking account of any relation between the science of Nature and the science of Scripture. In fact I believe that physical science well established on the basis of the Newtonian principles will eventually become the "handmaid" of religion. But the relation of the one to the other needs to be very carefully handled,—with cool judgment and well sustained argument. Inconsecutiveness of assertions, and incoherent references to authority, will certainly be taken advantage of by the acute sceptical spirits of the present day. Also before drawing inferences from relations of physical science to Scripture, it is plain that we ought to be quite sure of our science. From the contents of this work it will be seen that physical science is not the same thing in my view as in that of the authors of "Unseen Universe". In consequence of which, without disapproving of their purpose, I am obliged to dissent altogether from the arguments whereby they attempt to explain the mysteries of Christianity.

THE END.

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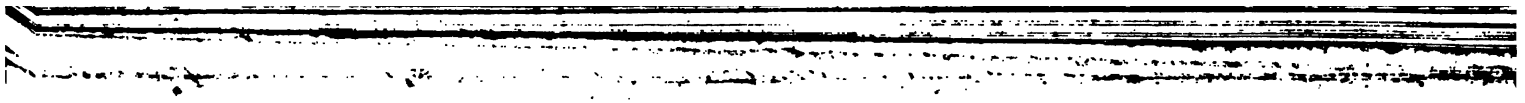

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